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SYMPOSIUM ON GEOMORPHOLOGY IN HONOR OF THE 100TH ANNIVERSARY OF THE BIRTH OF WILLIAM MORRIS DAVIS

(arranged by KIRK BRYAN)

Held in the Atwood Auditorium, Clark University, Thursday, April 6, 1950, 9 to 12 a.m., George B. Cressey, presiding.

After brief introductory remarks by the Chairman, the following six papers were presented:

Lawrence Martin: "William Morris Davis, Investigator, Teacher, and Leader of Geomorphology."
Discussion by: Roderick Peattie, Wellington Jones, J. K. Wright (by letter), and O. D. von Engelyn (read by K. Bryan).

C. A. Cotton: "Tectonic Relief."
(Read by title.)

Henri Baulig: "William Morris Davis, as a Master of Method."
(Read by title.)

Kirk Bryan: "The Place of Geomorphology in the Geographic Sciences."
Discussion by: W. W. Atwood, Jr.,* and Guy Harold Smith.

Arthur N. Strahler: "Davis' Concepts of Slope Development Viewed in the Light of Recent Qualitative Investigations."
Discussion by: George B. Cressey* and Louis O. Quam, also Bryan read a pertinent extract from Henri Baulig's manuscript.

Louis C. Peltier: "The Geographic Cycle in Periglacial Regions as Related to Climatic Geomorphology."
Discussion by: Richard J. Lougee, Joseph E. Williams,* Meredith F. Burrill,* and L. Dudley Stamp,* with reply by Peltier.

* Written discussion not received.

WILLIAM MORRIS DAVIS: INVESTIGATOR, TEACHER, AND LEADER IN GEOMORPHOLOGY

LAWRENCE MARTIN*

Honorary Consultant in Geography
Library of Congress, Washington, D.C.

A CENTURY ago there was born in Philadelphia the man who has, in my opinion, had a greater impact and a more permanent influence upon geomorphology—or physiography as many of us still say—than any person in the world has had before or since. A William Morris Davis centenary is a major event in American geography, geology, and meteorology. Evidence does not exist, of course, but no one can deny that the infant Davis's first word in 1850 was probably *"WHY?"*

* Col. Lawrence Martin studied in 1905–6 at Cambridge, Mass., under Davis, then Harvard University's Sturgis Hooper Professor of Geology, travelled with him in 1912 through United States during his famous Transcontinental Excursion of the American Geographical Society which W. M. D. organized, financed, and led, following plans announced to the A. A. G. in 1909, entertained him as a visiting lecturer at University of Wisconsin in 1911, saw something of him at G. S. A. and A. A. G. meetings, as well as in California in 1928, corresponded with him somewhat extensively between 1905 and the year of his death in 1934. Thus he knew Davis for 29 years.

The author expresses his indebtedness to Kirk Bryan, Walter C. Mendenhall, Derwent S. Whittlesey, and Harold Kemp for suggestions and criticisms. So also to Wellington D. Jones and to Roderick Peattie for their apposite remarks at Worcester, Mass., on April 6, 1950, as well as for permission to reproduce them herewith.

For convenience of readers, and without attachment of footnotes to individual references, the following publications concerning William Morris Davis are listed below.

(1) Reginald A. Daly. "Biographical Memoir of William Morris Davis 1850–1934," with bibliography, *Nat. Acad. Sci., Biog. Memoirs,* Vol. 23, 1945, pp. 263–303; (2) Charles F. Brooks and Lylyan H. Black, *Bull. Amer. Meteorological Soc.,* Vol. 15, No. 3, 1934, pp. 56–61; (3) Herbert E. Gregory, "A Century of Geology—Steps of Progress in the Interpretation of Land Forms," *Amer. Journ. Sci.,* Vol. 46, 1918, pp. 104–132; see also W. M. Davis's own biography of G. K. Gilbert; (4) Kirk Bryan, *Ann. Assoc. Amer. Geographers,* Vol. 25, 1935, pp. 23–31; (5) Douglas W. Johnson, introduction to W. M. Davis's "Geographical Essays," 1909, 800 pp.

By design the author makes no attempt to repeat the facts and tributes to Davis presented by the American scientists listed above, nor the biographical items and bibliographical details in these publications.

In addition to his publications, as may not be well known, Davis left a substantial body of unpublished geographical information in the form of (a) mimeographed laboratory directions; (b) printed examination questions: (c) printed bibliographical aids; (d) correspondence with fellow geographers including the present writer; and (e) manuscript drawings and maps; of these latter a group of some 85 products of Davis's talented mind and pen, given by him in 1930–31, are preserved in the Map Division at the Library of Congress and have recently been presented in photographic reproduction to the Harvard Geography Department by me. There may be substantial numbers of similar maps, profiles, and diagrams, as well as much Davis correspondence, at other American institutions and college departments and also in the personal files of Davis's students and other contemporaries in the United States and abroad, and in the offices of his several book publishers.

To my lifelong regret I was unable to accept Professor Davis's invitation to go with him in 1914, as his sole American companion, when he studied coral reefs, atolls, and other physiographic features in Oahu, New Caledonia, the Loyalty Islands, New Hebrides, Cook Island, the Society Islands, Fiji, Tahiti, New Zealand, and Australia, including a substantial portion of the Queensland coast inside the Great Barrier Reef. It would have been both a privilege and an ordeal to have been alone with Davis on such a journey; and yet he had his lighter, even frivolous, side as many of us know. For example he wrote me on July 23, 1914, while en route from New Caledonia to Australia, saying, *inter alia,* that while in the New Hebrides he had (and I quote) "bought most of a native's wearing apparel. . . . It consisted of a wild boar's tusk tied round his neck with a shoestring. When he took it off I feared he would catch cold, there was so little left on him—chiefly a crescent of tortoise shell in one ear, a safety pin in the other.—Such is life in the tropics."

The world was Davis's field of investigation. At one time or another during his busy and fruitful life he visited all but one of the seven continents. Antarctica alone was the continent he failed to study at first hand; but, at that, he published at least one paper on Antarctic geology and climate.

Adequate biographies and bibliographies of Davis have been published in America by Reginald A. Daly, Kirk Bryan, Douglas Johnson, and Charles F. Brooks. It is a record to make Americans proud. From it one may see that Davis the traveller was never a mere tourist. Throughout his busy and important life he was advancing his own education. He was observing, studying, and asking himself and other scholars and the landscape, "WHY?"

His foreign and American travel did more than to take him to overseas and domestic scenes (lamentably, of course, he never flew in airplanes and viewed the earth from the air); they also afforded him opportunity to know intimately not only his opposite numbers among geographers, geologists, and meteorologists and the terrains within which they lived and worked, but also the young men beginning or still progressing in geographical education.

In 1870, when 20 years of age, Davis had commenced a 3-year period of employment in the meteorological service of the National Observatory at Córdoba, Argentina, on the high plains sloping eastward from the Andes. This seems to have constituted his initial foreign travel and residence. He went around the world in 1876–77; to Turkestan with Raphael Pumpelly in 1903; to South Africa in 1905; to Australia by way of Hawaii and many small Pacific Islands, as already stated, in 1914; and to the coral reef islands of the Lesser Antilles in 1923. As visiting professor at the University of Berlin in 1908–9 and at the Sorbonne of the University of Paris in 1911–12, he studied European geomorphology and lectured with respect to it, as well as concerning the physiography of our own country. He visited or revisited the portions of Germany and France which interested or perplexed him, guided by local geological and geographical authorities, taking advanced students with him.

Before the opening of his Berlin lectures Davis conducted a nine-week geographi-

cal pilgrimage from Ireland to Italy in June and July 1908. At another time he led an excursion in the Alps and northern Italy. In subsequent years he revisited some critical foreign areas in Europe with students from the United States.

In America Davis travelled repeatedly and his publications, some of the great classics of United States physiography, are replete with answers to the question "WHY?" based upon map study, analysis of existing publications, visits to perplexing pieces of terrain (in some cases financed by the U. S. Geological Survey), and interpretation of the landscape.

At home in Cambridge, beside his Harvard teaching, Davis was also industriously writing books which ran the whole range of geomorphology, geology, and meteorology, for students of all ages except the elementary, and some of these paid him substantial royalties over the years.

In 1912, aged 62, Davis, as you all know, planned, financed, and conducted the famous transcontinental excursion of the American Geographical Society which brought to the United States 40 very distinguished and several promising younger European geographers. It was well and truly said in Minnesota during this excursion, by the great Archbishop Ireland, that there never had been gathered in the city of St. Paul a group of men who knew so much about the earth and owned so little of it. Our European guests were taken through representative portions of the whole United States, except New England, during the two months from August 22 to October 18 on a special train, in automobiles, on stage coaches, on steamboats, and on foot, with stops wherever geomorphology beckoned, not merely to cities or universities. Our guests were guided by Davis himself and some 90 distinguished American specialists. No international geological or geographical congress, I believe, has displayed a country so thoroughly or authoritatively. Nor will Davis's achievement a third of a century ago soon be equalled or surpassed.

Davis could take criticism as well as give it. Read, for example, what my own old teacher at Cornell, Prof. Ralph S. Tarr, wrote critically in 1898 about Davis's views and publications concerning the peneplains; and then observe how Davis replied the next year, effectually and without rancour.

Was Davis effectual with foreigners? Did he fully comprehend their lectures and publications and their field conversations and expositions? Indeed his linguistic ability was unusual for an American geographer. He lectured at the University of Berlin in English, at the Sorbonne of the University of Paris in English, but he had an adequate speaking knowledge of German and French, as was demonstrated during the transcontinental excursion in 1912. He undoubtedly acquired Spanish during his three years in Argentina. I have heard that he had a reading and speaking knowledge of Italian. Although he sometimes conversed in German, Davis wrote his "Erklärende Beschreibung der Landformen" in English, having it translated for him by Alfred Rühl; so also with his "Grundzüge der Physiographie," in which Davis collaborated with Gustav Braun, and also his "Balze per Faglia nei Monte Lepini" which was put into Italian by Fr. M. Pasanisi. The 1930 edition of Davis's "Elementary Physical Geography," published in Japanese and taken from

the 1902 or the 1926 edition of that work, was obviously put into Japanese by someone else. Naturally, then, Davis read geomorphological, geological, and geographical papers and books in European languages rather than having to depend upon translations or the summaries in English-language reviews. No great number of Davis's American contemporaries possessed these gifts.

No college professor ever taught me so much or did me so much good as William Morris Davis did. He pulled me up by the roots, pruned me, fertilized me, and set me out again in the garden of geography. At Harvard in 1905–06 Davis gave only one course in the first semester, and Martin was the sole student in it. The course was called "Advanced Physiography." Davis had been to South Africa during the preceding summer, attending an international geological congress, and arrived in Cambridge late. I had been to Alaska studying glaciers and glaciation; and then, as suggested by Davis the previous spring, and, financed by a surplus from the Sturgis-Hooper fund, had stopped for a short period along the Front Range of the Rocky Mountains in Montana.

At Cambridge I studied all available maps and printed reports, wrote essays and summaries, took them to the room in the Geological Museum where Davis met his one-man class in Advanced Physiography twice weekly, and read them aloud to my professor, as directed. Sometimes he heard me through without comment. Sometimes he made me repeat phrases, or paragraphs, or arguments which he found incomplete, ambiguous, or illogical, and, being a master of logic and himself somewhat acquainted with the physiography of the northern Great Plains and Rocky Mountain Front Range in Montana, he was always right.

On one happy day he interrupted my discourse while I was reading a descriptive quotation from an author whose name I had not yet announced. "What *does* the man mean?" said Davis. "I cannot conceive," he continued, "what the author had in mind when he wrote those words." He dissected the quotation at some length. Poor Martin had been hanging on to his chair and holding his tongue all this time. Then I said politely: "What *did* you have in mind when you wrote those words?" My quotation was from W. M. D. himself in a report on the Northern Transcontinental Survey under Pumpelly in 1877, some 28 years before, and my teacher had forgotten his own child. All he said, however, after a long awkward silence, uninterrupted by me of course, was *"Go on."*

Another remarkable morning in Advanced Physiography was the one when he arrived at the classroom happy and talkative, and said: "You are a very lucky young man, Martin, as I hope you realize. Here is Harvard University providing you with a private tutor." The Devil tempted me and I fell; "yes," said Martin, "yes, Professor Davis, Harvard University is even more generous since they pay me, as Edward Austin Fellow in Geology, a stipend to take the course." That time Davis laughed heartily.

No university professor, either at Cornell or Harvard, I repeat, ever did more for me than William Morris Davis did in 1905–06. During my second semester as a graduate student at Cambridge, Davis gave his wonderful course on the Physi-

ography of Europe, with lectures, laboratory work, and written reports. I was one of a substantial number of students in this course, and I received more information and less discipline; but I missed the ordeals of the first semester with its personal criticism and training.

Davis's impact upon education in America was, of course, not limited to his field studies, his publications, or his lectures at various institutions and his work as a Harvard professor. His subsequently famous students include Richard E. Dodge, Robert deC. Ward, Isaiah Bowman, J. B. Woodworth, Ralph S. Tarr, Walter C. Mendenhall, Albert Perry Brigham, Walter W. Tower, James Walter Goldthwait, and Philip Sidney Smith, several of whom took advanced degrees and themselves became members of the instructional staff at Harvard. Davis also lectured to students elsewhere at one time or another. Educational institutions favored by his teaching include Columbia, Leland Stanford, the California Institute of Technology, the Universities of Chicago, California, Oregon, Arizona, Clark, Wisconsin, and foreign universities previously mentioned. I wish we knew the total number of students who listened to or were trained by Davis in lecture rooms, class rooms, or in the field. The number of students in schools of all grades who read, studied, and recited from Davis's books and other publications over the years must add up to a gigantic total.

This great geomorphologist's genius in drawing was, of course, a major asset in his teaching. He made his facts and interpretations appeal to the eye as well as to the ear. The Davis block-diagram presented the facts of appearance and origin in ways which maps, photographs, and words cannot. They showed exactly how W. M. D. answered the question: "Why is the landscape the way it is?" But of course he also taught by spoken and written and printed words as well as by graphic presentation. In the course of his life he was author or co-author of more than 500 books and articles.

It is impressive to observe the extent to which Davis was given respect and praise in his own time throughout the world. He was an honorary member of 15 geographical societies; a corresponding member of 5; a corresponding member of 4 geological societies; a foreign member of 4 academies of science; he was an elected member of the American Philosophical Society of Philadelphia, and this was more than appropriate since Davis, the Quaker, was born in Philadelphia and lived in Cambridge near Boston; whereas Benjamin Franklin, another Quaker, and the founder of the American Philosophical Society, was born in Boston and did great things in Philadelphia. Davis was an elected member of the National Academy of Sciences in Washington, D. C., the Imperial Society of Natural History in Moscow, the New Zealand Institute, and the Geological Society of America. He was Acting President of the G. S. A. in 1906 and President in 1911. He founded the Association of American Geographers and was its President three times. He founded the Harvard Travellers Club and was its President from 1902 to 1911. He was an honorary life member of the American Meteorological Society. Davis deservedly

received medals and decorations from many geographical, geological, and other learned bodies in the course of his life.

The present speaker *raises* but cannot answer all the following questions: (1) Was the W. M. D. system of analysis, the terminology, the objective perfect? (2) Have many promising Davis pupils deserted geomorphology? (3) Has German open rebellion continued? (4) To what extent did Davis inspire the regional school in Wisconsin? Or influence the Chicago school of human geography? (5) Do we like *all* the trends in modern geography?

Cheerfully I acknowledge not knowing enough about post-Davis trends in geography to have opinions worthy of consideration. For the last 33 years I have been engaged in a dozen types of geographical work concerning which Davis, Tarr, Shaler, and my other teachers gave me no training. Engaged in trying to settle international or state boundary disputes and thus working for armies, diplomats, lawyers, members of Congress, persons interested in old, old maps and in great American personages who knew and used maps does not utilize the training which my college teachers provided. Nonetheless Davis taught me to be careful, logical, inventive, intellectually honest. In my own publications and my college teaching and other work I have *used* his principles, and I consider that we are all greatly indebted to him.

This centenary year enables us to render homage to a great American scholar, investigator, author, and teacher. It is my conviction that, when his bicentenary year arrives, geomorphologists, geographers, geologists, and meteorologists will still be rendering credit and giving praise to William Morris Davis, since the geographers he trained and their living and future disciples, American and foreign, are far from unimportant or uninfluential, and his published works are significant and sound. Unquestionably they will live.

Remarks by O. D. von Engeln (read in his absence by Kirk Bryan):

Nature Boy vs Nurture Boy. Colonel Martin has suggested that he does not know the answers to a number of questions properly raised in relation to W. M. Davis's preeminent career in various fields of geography.

There can be, of course, no question but that his contributions to the field of geomorphology far transcend what he did in other areas. Nevertheless, Davis always maintained that he was a geographer not a geologist. The geologists regarded this contention as a sort of aberration in which he could be indulged, yet be included in their own fold. The geographers, though not disowning Davis, on the other hand, have more and more abandoned his ways and approaches.

Colonel Martin's questions may be summarized in one: Is the geomorphic background an essential part of geography?

If my memory serves me correctly I recall that Davis made one of the early contributions to the elucidation of the Physiographic Regions of the United States. The course at Harvard on the Physiography of Europe to which Colonel Martin refers presumably had a content of such regional nature. This delimitation of Physiographic Regions, led to, or was a part of environmentalism.

This meant, in effect, that man is, as my title is intended to suggest, a Nature Boy. For, besides technical geomorphology, in the sense of land forms, the concept of Physiographic Regions includes the climatic factor and the vegetative response. Also the factor of location in the sense

of position. Accordingly, it was easy to reason that man's activities and altitudes were responses to his physical environment.

If one looks through the collection of Davis's papers bound up in the book "Geographical Essays" it will be found that he gave his support to the environmentalist concept. I pick out a sentence quite at random: "Give . . . school children . . . a clear idea of the forms of the land . . . that they may gain a better understanding of history past and present" (p. 107).

This, perhaps, is not entirely fair, but will nevertheless serve to represent a viewpoint which has since become anathema to geographers. From the Nature Boy thus envisioned they have turned completely away and for long have lavished all their affection on Nurture Boy.

Who is Nurture Boy? Well he is the creature of the human milieu instead of the physical. He is what he is because of his innate genius which differs with the race or the stock, the organization of society where he happens to be born, the customs and procedures of his forefathers. A proper study of geography is to discover what man has done in, with, and *to* the physical environment, rather than what it has done to him. Man is a Nurture Boy, pure and simple.

The irony of this is that now Nurture Boy bids fair to escape the geographers and elude them as finally as they deserted Nature Boy in the years past. What with the increasing unity of the world, travel by magic carpet, the word heard round the world, the scene televised, all Sears Roebuck items, not merely Standard Oil tins, available everywhere, what becomes of the darling Ashantis and their quaint ways governed by witch doctors, not by environment. They disappear in the general ruck, that's what! Nothing more of this kind is left on which to get a grant in aid to go and study and then to write about.

Thus we come back to the, if not eternal, at any rate enduring phenomena of geomorphology. Viva Davis! And who knows we may return to the concept that the circumstances of the human groups, when sociologically and psychologically they are all reduced to a common denominator, will best be described geographically in relation to the geomorphic and climatic environment. Hail the triumphant return of Nature Boy! Then we return to Davis's "Why?" in geography, demanding an analytical answer, instead of "What?" requiring merely description. However, probably fewer candidates will qualify as geographers then.

Remarks by Roderick Peattie:

You have heard Dr. Martin's brilliant paper on William Morris Davis. I can add but little as to his morphological theory. His writings were almost purely in physiography and meteorology, both important tools to the geographic science of today. But Dr. Davis was not always prolific in writing in spite of his tremendously large bibliography. In the Alumni Monthly of Harvard University, I believe dated 1939, there appeared an autobiographical article by Davis. There he tells how President Lowell called him in to explain to him that he was not doing research and that if he persisted in such a manner Harvard could do without him. So he set about to get some idea with which to write. T. C. Chamberlain had published a three volume work on the geology of Wisconsin which was reported as being excellent reading. Davis read the volumes and noted that Chamberlain in discussing the valleys of the Driftless Area spoke of one as young and another as old. From his use of these terms of age Davis evolved the concept of the cycle of erosion and his job at Harvard was assured.

But Davis had a much broader approach to geography than is indicated by his writings. He was constantly encouraging his colleagues to go beyond his written works. He was indeed the father of American geography. He defined the science and was merciless in making us stick to that definition during his life time. At times in the early history of this society it seemed as if he were an obstructionist so insistent he was upon speakers staying within his definition of geography. In student seminars he was so particular in insisting on the use of careful English that students were sorely put to it and indeed were almost in tears.

Later Davis insisted that geographical research should not necessarily have a practical application. Rather he looked upon geography as a science as well defined as other sciences and,

like the other disciplines, research in the subject should add to the sum of knowledge without regard to its practical implications. Indeed, if he were living today he would condemn our graduate schools in geography for the trends in their research, for he believed in the acquisition of knowledge as of first importance and would place that acquisition ahead of the dissemination of acquired knowledge. Such knowledge is best obtained not by instruction in lecture but by close association with those already proficient in research. These words are quoted from his speech at the opening of the Graduate School of Clark University. The speech is well worth reading again as indeed are all the writings of William Morris Davis.

Remarks by Wellington D. Jones:

When Colonel Martin publishes his excellent paper he may wish to elaborate somewhat two further points about Davis' work.

Davis had a heart of gold, but he made heckling a high art. He did so much good thereby that I believe his contribution should be emphasized. Davis rose to his feet innumerable times in AAG meetings, and suggested how papers *might* have been written and delivered. When these suggestions were followed, the papers always were improved. The first paper I presented at an AAG meeting was many years ago at New York, before I was a member of the Association. Davis spared me public criticism, but he invited me to lunch and tore my paper to shreds. I have ever been grateful to him for his frankness. Years after that, when I again met Davis, in California in 1926, he told me how he would have worded the titles of several papers I had published, and for good measure he added other suggestions. All this was to me priceless. And incidentally I have been encouraged to try to become a better heckler myself. Contact with Davis was almost certain to develop into intellectual battle—a battle for the truth and effective presentation thereof. Directly and indirectly, we of the AAG owe a great debt to Davis which I am confident he would wish us to pay only by more battling.

While I am on my feet I must say a few words on a second point concerning Davis and his work. In 1913, while I still was a graduate student, I took my bride to study a semester with Hettner at Heidelberg in Germany, financed by the profits of an incredible two years' field job in Northern Patagonia with Bailey Willis. During the two years in the field in Patagonia I had failed miserably at trying to apply Davis' ideas on geomorphology. With Hettner at Heidelberg, and Philippson from Bonn on a two weeks' filed trip in the Alps, I found out why I had failed. or at least part of the reason. Davis' ideas bore little relation to the facts of life, or more accurately, to landforms. While I was in Germany, Davis' ideas were sweeping over Germany as they had over USA like wildfire, but Hettner and Philippson refused to be engulfed in the conflagration. The result to me was that thereafter I looked at the Davis system with such a fishy eye that I never used it. Nevertheless, I always was and still am a great admirer of Davis for other of his ideas, not the least of which was heckling presenters of papers at AAG meetings.

Remarks by J. K. Wright (by letter):

Some Boyhood Memories of William M. Davis. Being the son of a Harvard Professor, I became acquainted with his colleague and friend "Mr. Davis" when I was about ten. I used to go over to his house and look on, fascinated, while he drew block diagrams. He gave me a copy of his "Physical Geography," which I read with interest if not complete comprehension—at least I enjoyed the pictures. In those days, I was Emperor of an imaginary country of which I made exquisite topographical maps with Higgins' red ink for the contours. Davis praised them but also was inclined to find fault with the physiography, as unconvincing. I sent him an official document (which he returned to me in 1932) signed "John Imperator," appointing him "King of Spineall, Duke of Calondit, Marquis of Simfrau, Earl of Celecticus, etc., etc.," as well as prime minister and "Chief Geologer." In 1923 many years later he wrote me from Cambridge:

"Your Imperial Majesty: It has been a great pleasure to your Prime Minister and Chief Geologer to receive your essay on Lat. and Long. in the Middle Ages. The evidence that it gives of your imperial majesty's extraordinary erudition has been much enjoyed. But in the matter of latitude I note with some astonishment that your imperial majesty makes no mention of the sun-

circle method of determination as devised centuries ago by your present prime minister in his early youth. . . ."

In 1906–1907 I spent the winter with my parents in Athens. While there I received a characteristic letter from my good friend in Cambridge:

"My dear Jack:

Your delightful letter, dated in various styles, gave me much pleasure. You may be sure that I have enjoyed your notes on physiography at different points on your journey and that your letter was read aloud to an interested audience at home. . . .

"I am wondering whether you tried to describe those cliffs at Boscastle and Clovelly according to the scheme—the only proper scheme, which you will find exposed in a certain textbook which you may have read. It has too long been the fashion with travellers like you to describe such cliffs anyhow of somehow. Just so, when you got to the Alps at Chamonix, did you see that the *Mer de Glace* had its end in a very well defined hanging valley whose floor was a thousand feet or more above the main valley in which Chamonix lies? If you did see that, you got ahead of me, for I was there in 1868 and in 1878 and never recognized the real significance of that extraordinary feature until 1899, when I saw it in a photograph."

He then proposed that I write an article for the *Journal of Geography!* On a drive southward from Athens along the seacoast I had noticed a double tombolo (Haliki) and recognized it as such from having dipped into "Physical Geography." Even if I had not wholly lived up to Davis's expections regarding the Devonshire cliffs and the *Mer de Glace,* I had described the tombolo for him in glowing terms, and he thought I should write it up. "The example is such a beauty that it ought to become known as a standard type of that sort of thing. Let me suggest that when you draw the diagram, you do not print the letters in ink but only in pencil, for I have less confidence in your printing than in your topographical drawing. I can then print over the letters in my own fancy style before sending the article and the figure to the *Journal of Geography* that Professor Dodge of Columbia edits. I am sure that he will be glad to have a contribution of this kind." This suggestion led to a sunny all-day trip in a sailboat from the Piraeus, with a crew who spoke Albanian. We lunched on the tombolo, walked all over it, sketched and photographed it. But at this point my memory fades. I was fifteen, and there were things to do in Greece that interested me more than writing for the *Journal of Geography.*

Toward the close of his letter Davis said that he was attending "an old man's gymnasium class in Dr. Sargent's gymnasium" and "keeping himself in pretty fine shape. . . . It is the funniest thing you ever saw to watch the old fellows go through the antics that they imagine imitate the graceful movements of the teacher. The other day one Professor M. Warren was so highly absurd that his neighbor Archibald Howe just lay on the floor and rolled over laughing. We do stunts at basket ball and that sort of thing too, and the result is that we are almost as tired laughing as exercising at the end of the hour."

I was not privileged to study under Professor Davis, as he was no longer teaching at Harvard when I was there. The interest that he aroused in me for his beloved physiography, however, led me to work under his successor, Douglas Johnson, something for which I am eternally thankful, even though in later years I wandered into other pastures.

My last letter from Professor Davis was dated "Pasadena, Calif., June 2, 1933." I had sent him a copy of a book on New England that I had edited, for which he thanked me with a few words of gratifying approval. "Is it not surprising that old New England has held out so well for so long a time? It had a good start of course, but it must also have [*sic*] had good stuff. Now the original stuff is diluted; some or much of it has moved away, perhaps the most enterprising part; much commonplace stuff has come in. Under such conditions it is all the more surprising that more disintegration has not taken place. . . .

"I am sorry not to have had time to read your book properly; but at present I am so busy trying to finish two of my own articles that it is impossible to look at any others except superficially. Time to stop as my machine is getting tired."

Manned by a lovable human spirit, it was a magnificent machine that did not tire until nearly the end. Professor Davis died Feb. 5, 1934, seven months after typing these lines.

TECTONIC RELIEF

C. A. COTTON

University College, Wellington, N. Z.

THESE remarks touch briefly on three kinds of tectonic relief feature: (1) humid-region fault scarps, (2) fault-angle valleys, and (3) first-cycle fault-line scarps. Investigation of these naturally follows the lead given by W. M. Davis, but calls for some extension of his analysis.

FAULT SCARPS IN HUMID REGIONS

It is a matter for regret that Davis did not ever feel called upon to investigate the peculiarities of fault-made relief in humid regions or to compare the humid fault-scarp cycle with the more familiar stages of development of initially similar features under arid and semiarid conditions.

In 1927 Davis[1] defined concisely his mature conception of the essential features of a fault scarp. After pointing out also a distinction between scarp-making, or, as he termed it, "Wasatch," faulting and non-scarp-making, "San Andreas," faulting (transcurrent, as it is now called), he remarked of Wasatch faulting that "the side of elevation is occupied by a more or less dissected mountain range, while the side of depression is occupied by an aggraded plain of mountain-supplied detritus." In the latter statement he ignored rather obvious alternatives, perhaps because he had in mind only the feature of examples in the more arid parts of North America. Broadly speaking, the distinction between the fault scarps of arid and of humid regions depends on the normal presence in the former of the aforesaid "aggraded plain," or bahada, fringing the base of the scarp.

A talus apron must fringe every fault scarp during its growth and infancy if the scarp is rapidly produced; but a scarp-foot bahada persists as a landform throughout youth and into maturity more especially under arid conditions. If the climate is humid, favouring the flow of vigorous rivers over the low lying parts of a block complex, the place of this is commonly taken quite early by a bedrock relief developed by mature dissection on the downthrow side of the fault, or by the eroded, or possibly aggraded, valley of a river flowing parallel to and in some places over the outcrop of the fault.

In either of these cases not only is the bahada absent, but the scarp is commonly without distinct spur-end facets. Even when the downthrown block is dissected by extended consequent streams, however, bluntly rounded facets are ranged in line along the fault at maturity, though these are not actually at the ends of spurs. If these midway facets on the spurs are prominent, the ensemble constitutes in effect a conspicuous scarp (Fig. 1). Where, however, they are small, owing either to relatively small fault displacement or to deep dissection both of the initial scarp and its foreground, they become mere jogs in the crestlines of spurs.

[1] *Geographical Review*, XVII (1927), 669.

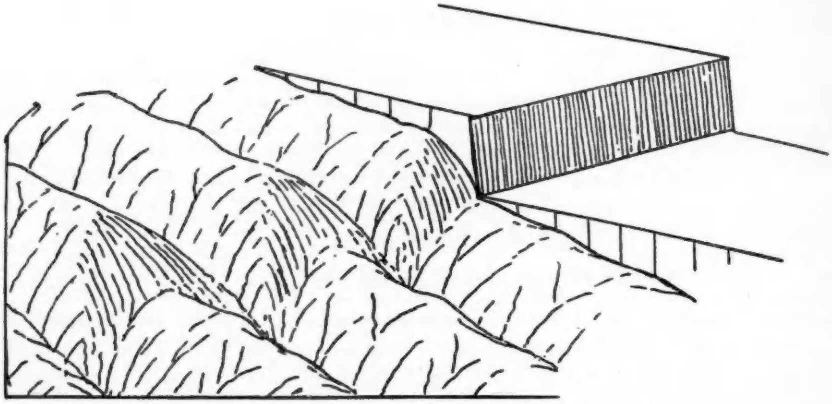

FIG. 1. Midway facets on spurs marking a fault line.

There remains the case of scarps laved at the base by rivers, a common case in New Zealand and one which may be reasonably expected to be common wherever precipitation is abundant in regions of active faulting of a kind which produces tilted fault blocks and related forms. Water must flow along the fault angles or collect

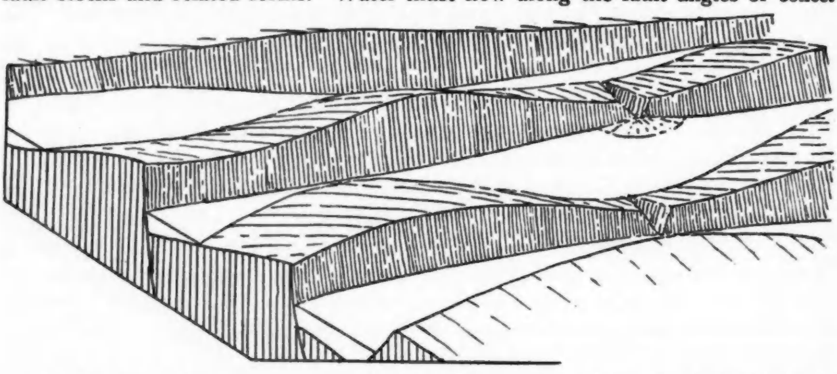

FIG. 2. Infantile land surface consisting of tilted and warped elongated fault-blocks in a humid climate. Extremely rapid deformation being assumed, water must flood the hollows. (In part after a diagram by W. M. Davis, 1930).

in them (Fig. 2), as shown by Davis in a diagram of "diversely tilted and warped
When such flow is concentrated on fault lines, the spurs of a dissected scarp, or perhaps an infantile scarp not yet dissected into spurs, will suffer mutilation by river erosion, thus losing some of their resemblance to Basin Range scarps such as are de-
blocks . . . in a humid region."[2]

[2] W. M. Davis, "Rock floors in arid and in humid climates," *Journal of Geology,* XXXVIII (1930), 1–17, 136–158, (Fig. 3).

picted in well-known diagrams. The scarp, indeed, comes more to resemble one wall of a rather straight river-cut valley; but this is not matched by a similar wall facing it. In extreme cases, exemplified by some growing or recently developed scarps in New Zealand, the whole feature is draped or fringed by a flight of river-cut terraces, which cannot be matched, and indeed have no counterparts at all, on the opposite bank of the river which has made them.

FAULT-ANGLE VALLEYS

According to Davis,[3] rivers consequent in the fault angles (or fault-angle depressions) in a landscape pattern initiated by upheaval of a mosaic of tilted blocks (Fig. 2) occupy "fault valleys." To distinguish these from some other consequent valleys on fault lines which it seems desirable to include in the general category of fault valleys they may be specifically termed "fault-angle valleys."

The question of whether or not bahadas must develop in front of all fault scarps leads to another: Is the conclusion of Davis that rivers consequent in fault-angle valleys are rare and exceptional applicable to all landscapes or is it correct only for arid regions? He reasoned that growth of bahadas, extending far out from the bases of fault scarps, must fend off from them any rivers which flow sooner or later over alluvium-buried downthrown blocks. Rivers may be found flowing more or less parallel to range-front faults but not so close to or strictly along them as to be regarded as consequent on the faults themselves. If, later, under new conditions of lowered base-level, such rivers degrade and excavate valleys in bedrock, these will not be "fault" valleys. The weakness of the potential rivers in new or developing fault angles—i.e. their lack of power to transport the waste supplied to them—which is here assumed, however, implies aridity or semiaridity of climate. On this implied assumption, therefore, Davis based his conclusion that "fault valleys," i.e. river valleys consequent in fault angles, must be rare, remarking: "When a stream valley is found to follow a fault line it probably has some other than consequent origin."[4]

Under humid conditions, on the other hand, the case which Davis never subjected to analysis, water collecting in tectonic hollows (Fig. 2) must normally flow away as vigorous surface streams, and parts of the consequent courses of these will hug the bases of fault scarps, notably in fault-angle depressions. Normally bahadas will not grow so extensively as to thrust these rivers away from the scarp bases, because the streams that are dissecting scarps and more certainly, the main rivers into which these discharge will be perennial and sufficiently full, at least at certain seasons or when in flood, to be capable of transporting the waste they erode and bring down from their headwaters. This is commonly the case, at any rate in maturity and even in late youth of the landscape of which the scarp forms a part, though it may not have been strictly true in the infancy of the scarp. From the foregoing it appears that the dictum of Davis, though acceptable for arid and semiarid, is without applica-

[3] W. M. Davis, "Nomenclature of surface forms on faulted structures," *Bulletin of the Geological Society of America*, XXIV (1913), 187–216.

[4] *Loc. cit.*, 1913.

tion to humid regions. Examples of rivers along faults in the fault-angles of a tectonic block pattern are common in New Zealand, and in many cases there can be no question that these rivers are consequent in origin.

Faulting produces such valleys the more easily if the prefaulting surface has little relief; and the dislocation of a plain of fluvial or marine deposition provides the optimum condition. In the more conspicuous New Zealand examples it is such a plain, the upper surface of a cover overlying a buried peneplain truncating harder rocks, that has been initially upheaved as blocks and broken anticlinal arches separated by fault-angle (partly synclinal) depressions at the commencement of the current major cycle of erosion.

Another kind of surface, a hilly, dissected surface, may, as Davis[5] described, be broken by faults with different results. Here, "irregular relief along the scarp base will cause any stream that may flow for a short distance along the fault line to depart from it sooner or later, and perhaps to leave it altogether." Faulting of such a surface could not, therefore, result in the development of continuous consequent streams along the fault lines. Here again another case may be taken into account besides that which Davis had in mind when he wrote. He thought only of faults being formed on new lines; whereas in actual cases faulting which breaks the land surface is commonly a renewal of movement on old faults or zones of dislocation. These are usually weak belts along which rapid headward erosion may at some time have excavated fault-line valleys—or perhaps fault valleys of some kind have been in existence on these lines prior to the most recent dislocation of the surface. Thus it comes about that active or recent faulting follows the lines of preexisting valleys instead of cutting across spurs and ridges, and new fault angles are formed along the length of these valleys, fault angles in which rivers can, and indeed must, flow, notwithstanding that the prefaulting surface had considerable relief. An example of such a fault-angle valley is that occupied by the Hutt River at Wellington, New Zealand.

TECTONIC FAULT-LINE SCARPS

Fault-line scarps developed by erosion along the lines of outcrop of faults that have been inactive since very ancient times are structural, not tectonic, features. Some geomorphologists, admitting only such multicycle forms in this category, rule that all fault-line scarps are contrastingly non-tectonic as compared with fault scarps. It is not clear whether Davis[5] when he adopted the description "fault-line" for scarps exposed by selective erosion at the outcrops of faults did or did not intend to include under this head any other than multicycle (which must be understood to include two-cycle) forms. His implied definition of the term as generally understood, however, makes it impossible to exclude those scarps exposed by erosion in the first postfaulting cycle. The significance of this is that the complex scarp marking the position of the fault line, of which the feature we are singling out as a fault-line scarp may be only a part, has not at this state ceased to be a tectonic form.

It is obvious from the context that the distinction in nomenclature between fault-

[5] *Loc. cit.*, 1913.

line and fault scarps was devised by Davis[6] primarily to separate purely erosional from fault-made scarps. Care in following Davis's lead in the use of these terms, though they are arbitrarily defined and not self-explanatory, makes for clear thinking and often helps greatly in the interpretation of the geological history of faulted regions. So well is the usage now established that it is no longer necessary to insert the additional word "erosional," as Johnson proposed, in the descriptions "fault-line scarp" and "fault-line valley."

Avoiding as usual the wording of a precise definition, Davis described in explanatory terms the kind of feature he was naming. He thus traced through two cycles the history of a form which he selected as the type fault-line scarp. In the earlier cycle a fault scarp consequent on dislocation of a surface henceforward subject to denudation was entirely destroyed by erosional processes, and the development of a fault-line scarp by selective erosion was delayed until a later cycle, perhaps until after a vast lapse of time. Though this multi-cycle fault-line scarp is the only kind Davis described, it is inconceivable that he failed to perceive—presumably he intentionally disregarded the fact—that erosion can excavate and reveal extensive strips of initially underground fault surfaces even in the cycle introduced by faulting, and that in some cases this process comes into operation very early in that cycle. He may have ignored this possibility because he pictured as typical fault scarps only the familiar bahada-fringed ones in the Great Basin; for foreground blocks which remain buried as in these cases under detrital deposits are in no immediate danger of dissection and excavation so as to expose fault surfaces, even if they are composed of rocks softer than those of the adjacent fault scarps.

A wider survey, however, suggests consideration of cases in which the foreground is not lowered below local base-levels. Conceivably it may even be raised by the initial movement, though to a less extent than the adjoining range from which it is separated by a fault. In either case it will be subject sooner or later, perhaps immediately, to erosional dissection and lowering. Examples of such cases are not uncommon in humid New Zealand; but clearly they are independent of climate. In these examples fault-line scarps have frequently been exposed by first-cycle erosion.

In one of Davis's published block diagrams he has portrayed successive stages of development of a faulted terrain consisting of soft cover overlying a harder basement,[7] a structure most favourable for the development of fault-line scarps. Strip C of this diagram shows a fault scarp of recent growth only very slightly degraded; after a vast interval the fault scarp is destroyed by erosion (strip D), the landscape being now worn down to the small relief of a peneplain on both sides of the fault line. This peneplain has soft rocks under it on the downthrow and hard on the upthrow side of the fault. Strip E shows the peneplain upheaved bodily; and F shows the exposure of a fault-line scarp by selective erosion in a later cycle. This is described in the caption as a "second cycle" fault-line scarp to indicate contrast not with any

[6] *Loc. cit.*, 1913.

[7] W. M. Davis, "The explanatory description of land forms," *Rec. de trav. off. a M. Jovan Cvijic*, Belgrade, 1924, 287–336, (Fig. 9).

first-cycle fault-line scarp described or implied, but only with the fault scarp shown in strip C, which is referred to in the caption as a "first-cycle" form. In such a diagram a strip might be introduced between C and D showing an important intermediate stage (Fig. 3, C′) in which erosion exposes a scarp along the base of the fault scarp of stage C because of the general lowering in progress on the soft terrain of the downthrow side. By this time the soft covering stratum will be eroded away from the high block, leaving a structural plateau. This will be marginally dissected along the scarp, but the dissection is not shown in the diagram. In numerous examples in New Zealand this stage has been reached, but the stripped plateau is a resurrected peneplain instead of the bared surface of a horizontal stratum as shown in Fig. 3, in which the structure shown in Davis's diagram is copied.

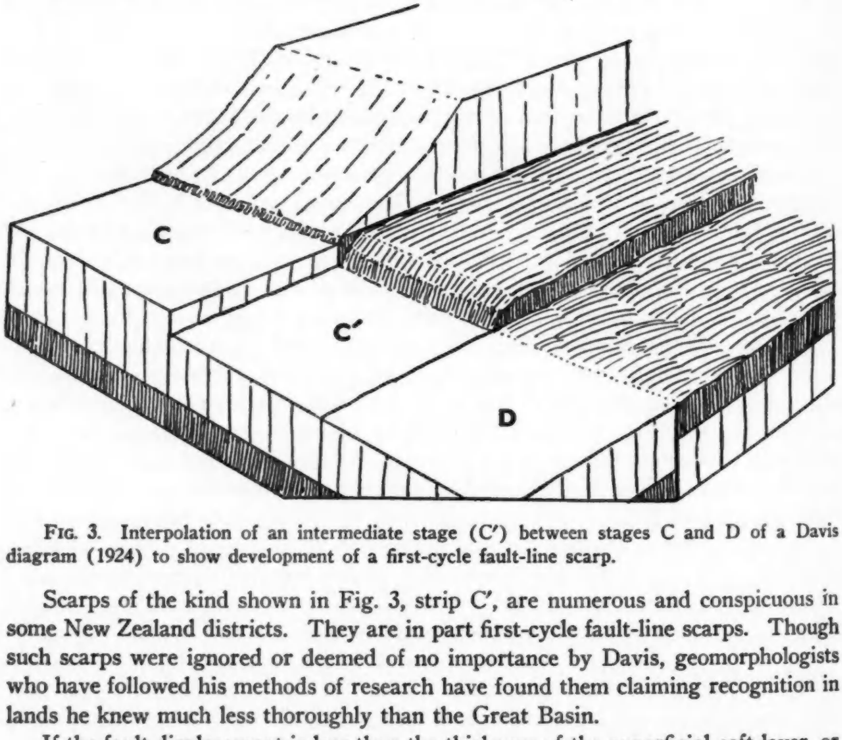

FIG. 3. Interpolation of an intermediate stage (C′) between stages C and D of a Davis diagram (1924) to show development of a first-cycle fault-line scarp.

Scarps of the kind shown in Fig. 3, strip C′, are numerous and conspicuous in some New Zealand districts. They are in part first-cycle fault-line scarps. Though such scarps were ignored or deemed of no importance by Davis, geomorphologists who have followed his methods of research have found them claiming recognition in lands he knew much less thoroughly than the Great Basin.

If the fault displacement is less than the thickness of the superficial soft layer, or cover (Fig. 4, A), the removal of this in part from the downthrow side of a fault, together with its complete removal from the upthrow side, will destroy the true fault scarp earlier present and reveal in its place a fault-line scarp. If, on the other hand, the fault displacement is greater than the thickness of the cover (Fig. 4, B), such degeneration will not be complete. This is the case in which the resultant form

is partly a fault scarp and partly a fault-line scarp, a combination which has sometimes been referred to as a "composite fault scarp" (Fig. 3, D).

In humid regions, where vigorous river transportation exports the debris of scarp dissection instead of allowing it to accumulate in front of the scarp, dissection of the foreground commonly takes place quite early in the cycle; and if, under such conditions, the foreground consists of a soft cover relatively downthrown as shown in Figs. 3 and 4, either complete degeneration to a first-cycle fault scarp or the development of a composite fault scarp is inevitable. It is combination of these circumstances that makes such forms common in New Zealand.

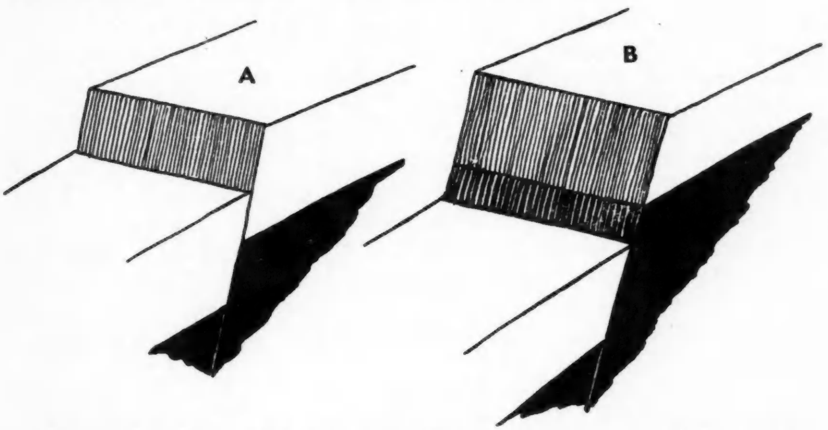

FIG. 4. Initial conditions which lead to early development of first-cycle fault-line scarps (A) and composite fault scarps (B).

The foregoing outline of first-cycle fault-line scarp development has been introduced to preface a claim that fault-line scarps are in many cases tectonic and that their significance in relation to the events of late geological history, if that history involves earth movements recently in progress, is just as great as that of "true" fault scarps tentatively reserving that description (as one author has done) for those intersecting a land surface that has undergone little change, and certainly has not been stripped of a continuous soft cover, in the postfaulting interval. Those fault-line scarps are tectonic which have degenerated from fault scarps in the comparatively short time required for a single postfaulting major cycle of erosion, as has been the case on many important tectonic lines in New Zealand. Such major features, though technically fault-line scarps, are best regarded simply as well-worn fault scarps.

WILLIAM MORRIS DAVIS: MASTER OF METHOD

HENRI BAULIG

University of Strasbourg, France

DAVIS'S contributions to particular problems of land sculpture are numerous and important. It seems, however, fair to say that his greatest service to geomorphology was to raise it finally to the status of an autonomous science by clearly defining its object, fundamental principles, and methods.

For Davis, the ultimate aim of geomorphology is and should be "the explanatory description of landforms." This raises at once the problem of its relationships to classical geology. Geology, he says in substance, is essentially a science of the past, its purpose being to decipher the physical history of the Earth, more precisely the history of its crust. Geography—viz. geomorphology—on the other hand, considers essentially its present aspect. Accordingly, the past should only intervene in the "explanatory description" in so far as it is indispensable to the understanding of the present. All "irrelevant" geological matter, however interesting it may be in itself, should be banished: it is even recommended that in descriptions the present (descriptive) tense should be preferred to the past (historical) tense. But even Davis's most faithful followers did not go quite so far as he would have led them. While he insisted on the "description," many students still prefer to focus their attention and efforts on the "explanation." Geomorpho-*geny,* Davis thought, should be subservient to geomorpho-*graphy:* a precept which he himself, very fortunately, often transgressed.

Geomorphology should ever be thankful to Davis for having freed it from the bondage of classical geology; but independence does not necessarily mean estrangement. Either science has its own way of looking at the past of the Earth, geology relying principally on deposition and diastrophism, geomorphology on the succession of erosion cycles. Both have their own—relative—geochronologies, which evidently should match and supplement each other. In other words, deposits, especially continental ones, ought, whenever possible, to be correlated at every stage with corresponding land forms, and *vice versa.* The task is truly a formidable one, and in its entirety beyond human power. It should, however, be carried on as far as possible, so that the history of our Earth may more and more appear in true perspective, with its erosional and its depositional phases described not only as successive, but also as simultaneous and interdependent.

* * *

It may be truly said that each science is characterized not only by its matter, but also, and still more so, by its methods. For instance, the word "geophysics," as commonly understood, covers quite a number of various lines of research, all concerned, it is true, with the Earth, but all using essentially the methods of physics: measurement, experiment (when possible), and calculation. Geomorphology also has its own methods: a general one in common with geology, and another peculiar to itself. Davis has greatly fostered the elucidation and formulation of both.

The general method is that of all sciences, physical or human, which have the purpose of deducing past causes from present effects. It necessarily uses analogical reasoning, based on the undemonstrable but indispensable principle of the permanency of Natural Laws. This principle, in simplest form, reads about thus: identity of causes means identity of effects, and reciprocally; from present causes it is possible to infer future effects; and from present effects, to infer past causes. It has been resorted to, more or less consciously, ever since man became aware of the law of causality: Herodotus, having realized that Egypt "is a present of the Nile," rightly concluded that a time had been when Egypt was a gulf of the sea. And similarly since the nineteenth century—and even now—the learned world has universally adopted the Lyellan principle of "uniformitarianism."

But to proceed from visible results to invisible causes, one has to go through a long chain of mental operations implying, successively and repeatedly, observation, abstraction, generalization, induction, invention (of an hypothesis, or preferably of multiple, competing, hypotheses), deduction (that is, invention again) of the consequences of these hypotheses, confrontation with the facts, final judgment. This method, in principle, is anything but new: it underlies, at least in semi-conscious form, every successful investigation of the past of the Earth. Logicians, from Francis Bacon on, have described it more or less completely, but always, as it were, from the outside, without a practical knowledge of the real working of a complex machinery. Scientists, on the other hand, have seldom cared to analyze and discuss the mental processes which had led them to their discoveries. Among geologists, it is true, G. K. Gilbert and T. C. Chamberlin had given precious advice.[1] But it was for W. M. Davis to do for geological sciences what Claude Bernard had done for biology *(Introduction à l'étude de la médecine expérimentale)*. His "Disciplinary value of geography" [viz. of geomorphology][2] is among other papers from his pen, a great classic of scientific method, and should be familiar to everyone engaged in geological or allied researches.

Such a piece of work cannot and should not be summarized; it must be read in full. Let us only call attention to a few points. Davis has, all through his long career, insisted on the essential rôle of imagination, reasoning, and judgment at every stage of the whole process. "Mental images," "mental counterparts of observed facts," "seeing and thinking, thinking and seeing," "weighing evidence" are some of his familiar expressions. Each of these expressions amounts to saying that the mind does not work with "facts," but with *symbols,* representing the *significant* characters of the facts; combined in various ways, they may, or may not, procure an adequate interpretation of observed facts and their interrelationships.

The method can, and, in Davis's opinion, should be practiced consciously and deliberately, until it becomes almost automatic: with, however, one exception. In-

[1] G. K. Gilbert, "The inculcation of scientific method by example," *American Journal of Science,* XXXI (1886), 284–299. T. C. Chamberlin, "The method of multiple working hypotheses," *Journal of Geology,* V (1897), 837–848.

[2] *Popular Science Monthly,* 1911, 105–119 and 223–240.

vention is, by universal consent, essentially a product of the subconscious part of the mind, from which it emerges unexpectedly, as a sort of revelation. But the revelation must be prepared by long and deep thinking. The great mathematician Henri Poincaré in a famous paper on "mathematical invention," after describing his own very striking experiences, interprets them in a suggestive manner. The preparatory thinking, he assumes, results in awakening many dormant ideas, mobilizing them as it were, and giving them a chance of meeting and forming original combinations. The greater the number, variety, and comprehensiveness of those concepts, in other words, the richer the mental fund of the individual and the more active his mind, the greater the probability of felicitous meetings. With Davis the fund was exceptionally rich, and the activity unceasing.

Invention, however, is inevitably confined within certain limits, within the circle of concepts familiar to the student and to his time: a heterogeneous mixture of reliable observation and sound inference with ill-founded assumptions, tacit postulates, and rigid dogmas. Davis himself did not escape the common fate: he had his own prejudices and deep-rooted inhibitions. To him, as to very nearly all geologists and geomorphologists of his time, the level of the seas had always been practically constant. Although the physical necessity of ample eustatic variations resulting from the formation and dissipation of Pleistocene glaciers had been clearly perceived by Charles McLaren as early as 1842, advocated again by Alfred Tylor in 1869, incidentally discussed in text-books, brilliantly revived by R. A. Daly, in connection with the problem of coral reefs, in 1910, Davis did not accept it until the next-to-the-last year of his life.

He rejected, *a fortiori* and without serious discussion, every kind of non-glacial eustatism. To him, any variation of level introducing a new cycle of erosion is due to unequal uplift of the land: so that, the equilibrium attained in the current cycle being upset, its forms, though persisting for a time, no longer evolve normally and are practically dead. Initiation of a new cycle is equivalent to termination of the former one. He never accepted—did he ever examine seriously?—the idea of several cycles, introduced by repeated sinkings of the sea level or by uniform uplifts of the land, and pursuing independently their courses, so that the same valley may offer evidence of several active cycles, simultaneous in development although successive in origin.

This led him into serious error regarding the interpretation of glaciated valleys in the Alps and elsewhere. Considering the gentle slopes of high valley-sides above the limit of ancient glaciation, he assumed at least equal maturity for the whole valley. On this basis he built a theory of glacial erosion—more exactly of the glacial bed—apparently coherent in itself, but inapplicable in a general way, for it does not account, as Emm. de Martonne (1910–1911) and others have shown, for the most striking characters of alpine valleys, viz.: junctions of tributary glaciers coinciding some with overdeepened basins, and some with bars ("Riegel"), steps in the longitudinal profile, "Trogschluss," stair-like glaciated shoulders . . . , all features implying not only structural differentiation, but also, in pre-glacial and inter-glacial

time, repeated attacks by fluvial erosion. Pre-glacial (and inter-glacial) valleys in the Alps were not mature, but young, or more precisely, composed of alternate segments, some young and some mature, with corresponding steepenings and flattenings of the longitudinal gradient, narrowings and widenings of the cross-section. The glaciers did not smooth these irregularities, but rather accentuated them, in accordance, as de Martonne has shown, with the tendency of moving ice to do more work where it encounters greater resistance. It is regrettable that Davis did not reconsider his theory in the light of these new conceptions.

Davis's general method, as, in fact, all geological inferences from the present to the past, suffers from a sort of congenital weakness: for one term of the analogist argument, i.e. the present, cannot be taken to represent the "normal" condition of the Earth. The Pleistocene, of which the present time is but a dwindling fraction, has witnessed repeated shifts in the general base-level, and repeated climatic changes, some of which have probably affected the whole globe, but not everywhere in the same way. Their effects are rather well known in glaciated and periglacial regions, much less so in lower latitudes, particularly in the contested belt between permanent aridity and permanent humidity. The question then arises: How far can concepts elaborated in humid temperate countries be transferred to different climates, colder or hotter, more arid or more humid? This is the problem of climatological geomorphology. Davis was well aware of its importance, and his contributions, especially to the morphogeny of semi-deserts, are fundamental. After describing "Physiographic contrasts, East and West,"[3] he parallelized "Rock floors in arid and in humid climates."[4]

But the problem has a more troublesome aspect. Passarge, who had worked especially in semi-deserts and arid steppes, claimed that present land-forms are not live forms ("Arbeitsformen"), but inherited features ("Vorzeitformen"), the product of more than one past climate. This is probably true in some cases, false in others, dubious in many, according to the constitution and resistance of the rocks, and to the amplitude and duration of the climatic fluctuations. So long as these matters are not cleared up by comparative studies, it seems safe to go on interpreting, so far as possible, present forms in the light of present climates, but with attention directed to aberrant features: for, as is well known, residues are often the receptacles of hidden truths.

"Procuring a terminology" was, in Davis's own terms, one of his main contributions to geomorphology: there is indeed no science without an adequate vocabulary. His terms are ingenious, pleasant, expressive, easily understood, easily transferred to other languages. Partly borrowed from common speech, partly artificial, his terminology serves well its purpose, though it cannot have the rigorous precision of the vocabulary of exact sciences: but geomorphology is no exact science. It may, however, be objected that his terminology is entirely genetic, forms being classified and named after their assumed origin. It seems that provision should have

[3] *Scientific Monthly*, XXX (1930), 394–415 and 500–519.

[4] *Journal of Geology*, XXXVIII (1930), 1–27 and 136–158.

been made for a distinct set of terms, denoting, without any implication as to genesis, some simple and obvious relationship, as for instance between structure and direction of the drainage: monoclinal, cataclinal, anaclinal, as applied to valleys, are rarely synonymous with subsequent, consequent, obsequent, for first-cycle landscapes in which they exist, appear as investigation progresses to be more and more exceptional.

* * *

Thus far, Davis's method is not essentially different from that of classical geology. But the notion of an ideal cycle of evolution for land forms is truly an invention of his own, one through which geomorphology definitely attained an independent status. Long before him, it is true, indeed from classical Antiquity on to Charles Lyell, some of the processes involved in land-sculpture had been observed, some simple forms, subject to rapid changes, such as volcanoes, sand dunes, sea cliffs, meanders, had been described and correctly interpreted. But "the combination of all forms in a well-organized whole was not yet accomplished."[5] Davis undertook the explanatory description of whole landscapes as combinations of interrelated forms.

True also, the scientific thought of the nineteenth century, particularly in its latter part, had been dominated by the concept of evolution, that is of gradual though not necessarily continuous, changes, all tending in some definite direction. This concept, as applied to land forms, was present, for instance, in L. Rütimeyer's description of Alpine valleys as younger or older (1869), and still more in Powell's theory of "reduction to base level" (1875). Davis's conception he himself presents thus:

> ". . . the idea that certain groups of forms may be arranged in a genetic sequence based upon structure, process, and stage, and the further idea that the different form-elements of a given structural mass are at each stage of its physiographic evolution systematically related to one another. . . ."[6]

This conception is strikingly similar to that of plant-sociology, to which any vegetal formation is both an association, a biocoenose, whose parts are all interdependent—a truly geographical notion—, and a stage in a progressive (eventually in a regressive) series—an evolutionary point of view. Similarly, every element of form in a landscape, to be fully understood, must be conceived both as correlated with adjacent elements, and as a term in a continuous development. Given initial form, structure, position relative to base level, climate, the forms will pass through a predictable and irreversible succession, but under two essential conditions: first, a state of equilibrium must have been attained between processes and structure, between forces and resistances: a moving, dynamic equilibrium, to be sure, but, for that very reason, an enduring one;—and secondly, climate, land, and base level must remain at least approximately stable during the whole development.

[5] W. M. Davis, "The physical geography of lands," 1900. See *Geographical Essays*, p. 75.

[6] "Peneplains and the geographical cycle," *Bulletin of the Geological Society of America*, XXXIII (1922), 594. Cf.: "[*Die Methode*]. . . , die bestimmte Formengruppen in ihrer natürlichen Entwicklungsfolge vorführt," *Der erklärende Beschreibung der Landformen*, 2. Aufl., 1924, p. XXVI.

This conception has been assailed on various grounds. The question has often been raised whether stability has ever and anywhere lasted long enough to permit the development of a complete cycle, whose duration Davis himself tentatively estimated at twenty to two hundred million years! It must be admitted that grave doubts may subsist on this point: for it becomes more and more probable that peneplains of continental extent were not formed, as Davis assumed, by slower and ever slower reduction of all slopes, but that either they are made up of elements—facets—of different ages intersecting one another at a small angle, or that they were modelled under special conditions of climate, allowing local levelling to proceed simultaneously in many different parts of a large area, so that general—or nearly general—planation might be attained without reduction to one and the same base level. The question is still open, as to the precise characteristics and possible extension of such climates: humid tropical? subarid tropical? arid tropical? subtropical? And will the Davisian cycle work successfully if applied, in modified form, to these apparently aberrant cases? Some will insist on the differences, some on the similarities: both tendencies are legitimate, and diverging ways may finally lead to the same truth. At any rate, the cyclic scheme accounts well for the development of local (or partial) peneplains under a humid temperate climate.

Davis has been repeatedly criticized for assuming rapid, practically instantaneous uplifts of the lands, and ignoring the effects of erosion during the uplifts. But he aptly replied that he had done so mainly for simplicity's sake, and he referred his opponents to some passages of his works where he had made allowance for mature valleys remaining so during very slow uplift of the land. We may add that his critic's premises were not always sound. One of them, for instance, claimed that entrenching of meanders, being a gradual process, implied persistent uplift. But, given a rapid *uniform* uplift or a negative eustatic movement, their effect is gradually propagated upstream, so that sections of the river and valley are rejuvenated successively, long after the movement has ceased.

One of Davis's principal opponents, Walther Penck, thought it possible to deduce the rate of past uplift from the shape of valley sides: profiles respectively straight, convex, and concave, should denote uniform, accelerated, and decelerated uplift. But this is mere illusion. In the first place, one should disregard young, irregular, structurally controlled profiles, and consider only the graded valley-sides of maturity. Now, Penck's fundamental error consists in believing that such regular profiles are composed of a number of distinct elements *successively developed.* Whereas a graded slope, like any other form of equilibrium, is being constantly remodelled *throughout its whole extent:* all its parts are mutually dependent, *all adjusted to present conditions, and hence totally independent of past events.* The observable forms express equilibrium, but are silent as to how equilibrium was attained.[7]

This leads us to the general conclusion that continuous evolution, proceeding at a uniform or slowly changing rate, appears to us, observers of one day, as equivalent

[7] See H. Baulig, "Sur les Gradins de Piedmont," *Journal of Geomorphology,* II (1939), 281-304.

to invariability: just as instantaneous perception, unaided by memory, would not tell us of the lapse of time. If the history of land forms is legible, it is because each of the successive cycles was introduced so rapidly that forms of the previous cycle, not having had time to become adjusted to changed conditions, pursued their own career independently—or were buried at least for a time. Owing to this fortunate circumstance, the past, as it were, still continues in the present, or else is preserved for future observation. Thus, Davis's assumption of rapid "uplifts" is necessary not only to account for the obvious discontinuity of forms, but also to make the history of landforms intelligible.[8]

* * *

To John Leighly,[9] the Davisian cycle consists in "the setting up of a developmental series of phenomena . . . , a method of ordering facts for which a mechanical explanation is unavailable." And "Davis's great mistake was the assumption that we know the processes involved in the development of land forms. We don't and until we do we shall be ignorant of the general course of their development." This is radical criticism, which should not be received without examination.

In the first place, mere sequence is certainly no explanation when the successive terms appear haphazard, without any perceptible connection. But regular, orderly sequence is, in itself, at least a beginning to intelligibility. What, after all, is causality but constant sequence? And were not many physical laws discovered and firmly established long before the phenomena could be reduced to molecular and atomic processes? But development is much more than mere sequence. In true developments, such as are assumed in the theory of the cycle, each term proceeds from the preceding and virtually contains the following; the whole chain, from beginning to end, is evidently subject to one and the same causal law, and discovering the law amounts to understanding the phenomenon *as a whole*.

The law, it is true, conceals certain mechanisms, which can be investigated, so to speak, at different scales. Davis did not study the detail of the processes, he did not analyze them farther than seemed necessary to account for the resulting forms, but was satisfied with seizing them globally, in their visible effects. One may, of course, wish to go much farther and to penetrate into the intimacy of the processes, so as to be able ultimately to reduce them to some simple and general laws of mechanics and physics. This is truly a formidable task: not only because forces and resistances at play in geomorphological phenomena are many and various, but also because they are so interrelated that any variation in one of the factors involves variation in all the others—which is precisely the essence of dynamic equilibria. This has been abundantly demonstrated by the many unsuccessful attempts at discovering the law connecting the shape of the river bed with discharge, slope, volume and

[8] Contemporaneous deposits, on the other hand, may, and must often have registered morphological events which have left no trace in the present forms: one more reason for not severing the story of forms from that of deposits.

[9] "Symposium on W. Penck," *Annals of the Association of American Geographers*, XXX (1940), 224–225.

nature of the load, etc. It is, of course, possible to analyze such complex processes mentally, and even, within limits, materially; but it may be questioned whether they can ever be decomposed into their elements without losing some of their essential characters: for the "ensemble" is interplay, and not mere summation.

Experimentation, Davis thought, is *at present* of little avail in such matters. One can, indeed, reproduce, or rather imitate, some simple phenomena at their natural scale: but this is hardly more than making observation and measurement easier. When it comes to experimenting at a reduced scale, all the "dimensions" of the phenomenon, lengths, volumes, densities, viscosities, velocities, must be adjusted in such a way as to maintain their natural ratios: this cannot be done, in general, for all the variables involved. At any rate, experiments of this kind are very different from the much more simple and abstract procedures of physical laboratories, and seldom lead to definite conclusions.

Davis, who had studied mathematics, never made use of it in his geomorphological work: not a single formula is to be found in his some three hundred papers on land forms. This is all the more significant as it strikingly contrasts with the general tendency of the nineteenth century: orography originated in Europe as orometry; Albrecht Penck's *Morphologie der Erdoberfläche* (1894) is full of equations, whereas they are practically absent from his later work. Mathematics is an infallible transforming machine, but it cannot return more than it has received. Before a useful equation can be written, the phenomenon under study must have been qualitatively understood, analyzed into all its essential parts, with their interrelations clearly defined. Then each factor must be assigned a certain value, which can rarely be determined by exact measurement. Engineers will always use more or less empirical formulas for practical purposes. But there remains to be proved that mathematics has ever revealed in geomorphology an actual relationship that had not been discovered without its aid.

A more general and speculative question may be raised. How is it that complex, intricate processes engender simple, harmonious forms? Is the explanation merely of a statistical nature, just as, for instance, the disorderly motion of gaseous particles is expressed in the simple Boyle-Mariotte law? Or shall we presume, in harmony with recent progress in physics, that matter behaves differently at the atomic, at the molecular, at the microscopic, and at the macroscopic scale? Are there in nature real discontinuities, "uncomformities," with different laws ruling at each level? If it were so, we could not expect even complete analysis of processes to reveal the law of forms. These then should be viewed as "wholes" ("comprehended"), rather than decomposed ("explicated").

Whatever success may attend the study of processes, the interpretation of forms will remain the ultimate aim of geomorphology. To this end, the Davisian method has not, thus far, been superseded.

THE PLACE OF GEOMORPHOLOGY IN THE GEOGRAPHIC SCIENCES

KIRK BRYAN
Harvard University

DAVIS' GEOGRAPHIC PROPAGANDA

TO William Morris Davis the modern American geographer owes tribute. He preached the gospel of geography on all occasions during his long and active life: geography in the elementary schools, in the high schools, in the colleges, and in the universities. As an advocate he had no peer. Devoted and dedicated to the analysis of land forms, he also spent much time and thought on the methods of teaching the elements of physical geography: the earth in space, its diurnal and annual rhythms, the tides, weather phenomena, and other elementary facts and processes of our home, the earth. Moreover, he continuously advocated investigation of the relations of all the primary factors of physical geography with the life of man. He was consistently friendly to the cause of "human geography."

Now, before this Association of which he was the founder and thrice president, it seems proper to consider the place in geography of his main interest: physiography, or, as we currently say, geomorphology. Davis' advocacy of the study of the interrelation of man and his environment has triumphed. Except as propaganda, his own contribution was small, but the missionary zeal of Davis and his contemporaries has borne fruit. Today an overwhelming majority of our members are principally concerned with various elaborations of "human geography." Whereas when this Association was founded, the majority of the leading members were geomorphologists and climatologists trained largely in the physical sciences, the present membership is drawn from the social sciences or has been trained in departments or "schools" of geography in which physical geography is only a minor phase of the curriculum.

The present position of geomorphology presents a paradox. As Russell (1949) points out, there is at present an enormous and world-wide interest in geomorphology. We are having a revival which would warm the heart of Davis and would also yield him many misgivings, as I shall point out. Yet there is among the geographers much indifference and even a modicum of hostility. Perhaps this is too blunt a statement, or one which fails to reflect the situation. There is even some evidence that the attitude sensed by Russell and myself may be more apparent than real. A check of the recently published membership list (*Professional Geographer,* N. S. Vol. I, supplement Nov. 1949) shows that 105 members of the association listed "geomorphology" or "physiography" as one of their "major interests" or are known by me to be geomorphologists. Many others list "physical geography" as a "major interest," an expression which would indicate that they have at least some interest in geomorphology.

We should therefore inquire as to this interest. Is it simply traditional and an

aftermath of Davis' propaganda? Is it merely man's inherent interest in "Natural History" which takes so many forms and erupts in "Nature Study" groups and Mineral Clubs? Or is it soundly based on inherent necessities in the study of other geographic fields?

GEOMORPHOLOGY AS A PART OF GEOLOGY

The historian of science will regard the career of W. M. Davis with interest. Appointed Assistant Professor of Physical Geography at Harvard in 1885, he thereafter claimed to be a geographer. Consistently during the development of geomorphology (physiography) he protested that the objective was the "clear, the genetic description" of the land. His American followers, although faithful to his tenets and using his methods and terminology, considered themselves geologists. Davis himself was president of the Geological Society of America and honored by the Penrose Medal of that Society. Johnson (1929) and Campbell (1928), both presidents of this society, were adherents of his geomorphic doctrines. Yet each of them in his presidential address before this Association states clearly that geomorphology is, in working method, a geologic science. They subscribe to Davis' contention that the results, or at least part of the results, are geographic.

To the greater number of American geologists the question of the geographic status of geomorphology seems unreal. They accepted and honored Davis and his followers. They held, and still hold, that geomorphology is a part of general geology and a working method in historical and tectonic geology. Every major graduate school has a geomorphologist (sometimes considered a glacial geologist) on its staff. Nearly all geomorphologists are members of geologic departments or of geological surveys. Many are also known for their contributions to other branches of geology.

In Europe the situation is much more confused. Nearly all geomorphologists are Professors of Geography. Some serve on Faculties of Science and some on Faculties of Arts and Letters. Many, in addition to their work in geomorphology, contribute also to "human" geography. On the other hand, many European geologists are sealed off from contact with geomorphology. Not only do they fail to take courses in the subject as part of their training, but they are enlisted in feuds between professors of geology and geography. Thus many of the geologists ignore geomorphic methods or use them reluctantly. Geomorphologists are not only on the defensive as to geologists, but are also sensitive on the geographic side and tend to over-argue their claim that geomorphology is geographic.

Thus Mortensen (1943–44) holds that the difference between geologists and geographers lies not in working methods by which, as claimed by some, the geologist studies the underground and the geographer the surface, but in the objective. No scientific discipline can be denied any pertinent method, he says, and thus he admits that geographic geomorphologists carry on work identical with that of geologic geomorphologists. The difference, according to Mortensen, lies in the objective. Geology is an historic subject, dealing with the past and particularly the conditions of the past. Geography deals with the present and particularly with the forms of the

surface as they now are. "The objective of research for the geographer is the present-day form picture and not the processes of the past however important." But as these processes of the past must be thoroughly investigated in order to understand the present-day form, the geographer is left in the position of publishing much geologic material and thereby qualifying as a geologist or of suppressing his evidence and therefore writing "geography."

Fortunately for us in the United States gemorophology has attained a place as a geologic science and most geomorphologists hold positions on geologic faculties. The large graduate schools all maintain courses in the subject. On the other hand, many geomorphologists pursue other geologic specialties and may in fact spend most of their time on other phases of geology. There is, therefore, less hypercritical argumentation to the effect that geomorphic research is geography, although many follow Davis in believing that the principal objective and the real value of their work is geographic. It is no exaggeration to say that the leading geomorphologists are friendly to geography, both physical and human. They are not only willing but anxious to make their specialty as useful to geography as it obviously is to geology.

THE RÔLE OF GEOMORPHOLOGY

It seems, therefore, pertinent to reconsider geomorphology. What is the function of the subject? Is it an independent science or a sub-science? Natural as it may be for exponents of a subject to claim independence and to extol the unique virtue of their favorite field of effort, geomorphology can hardly claim the pre-eminence of separateness.

The forms of the land, whether erosional or depositional in origin, are the result of processes still current. Thus a large effort has been and is still being made to understand this process. Such studies involve rain and rivers, the diurnal and seasonal changes in temperature, moisture of the air, the alimentation and movements of glaciers, and, in short, the whole array of phenomena included in physical geography. However, weathering, erosion, and sedimentation occur with the cooperation or the hindrance of the vegetative cover. Thus the plant and even the animal world are also involved. In fact, a wide array of studies not only in physical geography but also in bio-geography are of interest to a geomorphologist and he may himself be a contributor to these fields.

If land forms were solely the product of current process, these studies of their origin might easily be classified in physical geography or more properly perhaps in dynamic geology. The most interesting and in fact the larger part of our present topographic forms are, however, inherited from the past. Davis and his contemporaries may have exaggerated the antiquity of some of the forms, but nevertheless the surface of late Pleistocene time is largely still with us and well-preserved forms of late Tertiary age are relatively common. Thus the essence of geomorphology is the discrimination of the ancient from the modern. It is, therefore, essentially a branch of historical geology (Bryan, 1941).

That this discrimination requires elaborate new studies of current process is ob-

vious. In fact, the progress of geomorphology has been made by a succession of movements back to Nature as the great teacher. Thus Gilbert laid the foundation for studies of the ancient shore lines of Lake Bonneville by his studies of Lake Superior (1885). Our knowledge of loess goes back to von Richthofen's study of current dust storms in China (1877). In fact, the German school of travellers and physical geographers have had an enormous influence on modern interpretation. Davis, himself, rested his interpretation of rivers, particularly the concept of the graded river, on studies by Italian and French engineers. Instance after instance can easily be cited. It is neither startling nor unprecedented that new studies of the Mississippi and its delta should lead to new interpretations of modern and ancient forms in alluvial morphology, as Russell (1949) points out.

One may raise the question as to the objective of geomorphology. Does it need any other objective than to understand land forms? After all, every object in Nature excites curiosity and on purely intellectual grounds deserves study. Hence it is possible to advocate an ideal geomorphology or, one might say, a "pure" geomorphology. This would be a study merely for the sake of study. It is, however, an error to believe that land forms can be interpreted merely on their form, i.e. on morphology alone. One must agree with Russell (1949) and Mortensen (1943) that the full resources of geology and all its methodology must be available. If ideal geomorphology involves the full panoply of geologic study, it is obvious that one can not hold aloof from historical geology. The first and most natural application of geomorphic study is to the history of the earth. From this standpoint geomorphology is a working method inside of the mother science of geology, as Johnson (1929) insisted. Many events are recorded only in ancient and now dissected land forms, whose analysis and correlation are of prime geologic importance.

Geologic Applications of Geomorphology

The use of geomorphic methods and analysis is ingrained in American geology. Many exponents of these methods consider themselves specialists in other fields. However, they use geomorphology with freedom as they might use any other method.

Sedimentation. Renewed interest in the deposition of sediments is a feature of the past 30 years. As sedimentation is the end result of erosion, the distinction between this field and geomorphology is gradational. The laboratory techniques of sedimentation (and sedimentary petrology) are unique, but all general argument as well as the application of sedimentary studies to stratigraphy borders on and depends on geomorphic analysis and results.

Tectonics. As the Tertiary and later uplifts of the Earth's crust have not yet been wholly reduced they are expressed in land forms. Analysis of these forms affords data on the form of uplift, on its separation into phases, and on its place in time (as an example, Putnam, 1942). As displaced remnants of stream deposits, lake beds, etc. may be preserved, sedimentary and stratigraphic studies are frequently combined with those that are morphological.

Economic Geology. The study of placers, especially those of ancient and now

deserted river beds, involves geomorphology, as amply demonstrated by Lindgren's "Tertiary Gold-bearing Gravels of the Sierra Nevada" (1911). Similarly placers in beach deposits ancient and modern involve geomorphic studies. Residual deposits of manganese, iron ore, and chrome can be studied by elaborate chemical and mineralogic means, but the geomorphic history of the area usually yields the reason for the formation and also for the preservation of such deposits. Engineering geology involves two great types of problems: (1) foundations of structures and (2) regulation of current processes in rivers, on shores, in dunes, etc. Geomorphology, like any geologic tool, is useful in the multitudinous problems of the first category, but in the second it furnishes both the base data and much of the methodology in the gaining of new data.

Interpretation of the Past. Every unconformity is the record of an erosion surface. How are these gaps in the geologic record to be filled with vivid interpretation of an otherwise unrecorded episode? The erosion surface must be described, pictured, and fitted into the numerous categories of our geomorphic catalogue. From the standpoint of geologic history this service is perhaps the most vital performed by geomorphology.

Glacial Geology. The invention of the Glacial Theory by Agassiz and his Swiss associates (1840) was largely based on geomorphic observation and analysis. In front of the existing glaciers of the Alps lay U-shaped valleys, with scratched and grooved surfaces, piles and mounds of débris, and erratic blocks. This topography could only be produced by ice; these materials could only be carried by ice. They inferred that the ice had once extended far down the valleys beyond its present limits. Thus Agassiz converted his New England associates to the glaciation of the New World: "If we should see this (this topography, these scattered rocks) in Switzerland we would say that the ice had been here." In the development of glacial geology, weathering phenomena, stratigraphy, sedimentary analysis, and other methods have come to the fore. But the main dependence for discrimination between "drifts" of different ages, between differing deposits is geomorphic. We have those who hold that glacial geology is unique and that the multiplicity of methods involved remove it from the category of geomorphology. On the other hand some of our European colleagues insist on their identity as geomorphologists although most of their efforts are expended in glaciated regions and they use every known refinement of sedimentary analysis and stratigraphy in their work. Glacial geologist and geomorphologist may insist on a differential treatment, but the geologic world will doubtless consider that this is only an argument between Tweedledum and Tweedledee!

Paleogeography of the Pleistocene. The recognition that the Pleistocene, including the Present, is a period of cold as compared to most of geologic time is not really new. Not only was the Pleistocene colder, but it had a highly fluctuating climate of which the actual ice sheets were only one of the consequences. Glacial topography characterizes only part of the Earth, although to many of us a very important one. Nevertheless the sharp demarcation between erosive and depositional process in the

Tertiary and in the Pleistocene is even yet not wholly appreciated. It is, however, inspiring new viewpoints on old problems. In a zone south of the ice sheets and in mountain areas surrounding mountain glaciers the intensive frost-action and strong winds affected the topography of the periglacial zone. A growing literature treats of these phenomena—Smith (1949), Denny (1938), Cailleux (1942). Intensive frost-action overloaded streams beyond the glaciated area and flood plains were built up to be dissected in interglacial periods (Peltier, 1949). Similarly, in arid regions, the changes of climate of the Pleistocene produced lakes in undrained basins, terraces on streams, and landslides in areas now stable. The vegetal and animal population shifted back and forth with these climatic changes leaving fossils whose interpretation affords not only measures of these changes but also data on the chronology. A happy combination of stratigraphic, sedimentary, and paleontologic methods combined with a geomorphic viewpoint has clarified the Pleistocene history of Kansas (Frye, Swineford, and Leonard, 1948; Hibbard, 1949). In this sequence a volcanic ash (Pearlette) widely spread over the Great Plains and now identified from Nebraska to Texas has afforded a useful time marker. Ash showers have proved to be markers in Oregon (Allison, 1945), Argentina (Salmi, 1944), in Iceland (Thorarinson, 1949), and in the Yukon (Denny and Sticht, in press). Useful as these ash showers are as time markers, their more complete study will yield data on the wind direction of the past and hence furnish meteorological data for the restoration of Pleistocene climate.

Geology of Archaeology. The use of geomorphology and all the other methods and data of Pleistocene geology in the study of early man is now a well-developed specialty. The problem is usually two-fold: what is the date of a given site and what were the paleogeographic circumstances under which the ancient people lived? The ordinary geologic chronology at any one place is for this purpose inadequate and consequently elaborate studies involving all phases of geomorphology, sedimentation, stratigraphy, paleontology, and paleobotany are necessary (Johnson, 1942 and 1949). The second phase is much more closely related to human geography. In fact it becomes a study in paleogeography in every sense. Thus Hack (1940) has studied the modern farming methods of the Hopi and applied this knowledge to estimates of the state of their farming as far back as their history goes. The Hopi and other southwestern farmers were affected by the epicycle of erosion which began about 1200 A.D. and was coincident with the breakup of villages and general changes in population in the following 100 years (Bryan, 1941). The spectacular changes in climate of the late Pleistocene to the present and coincident fluctuation in Lake Tecoxco have been outlined by de Terra (1949). Movius (1949) has summarized the climatic history and concomitant changes in land and sea during the development of man in southeast Asia. In fact, we have now a whole literature and a group of devoted students of what may be regarded as the paleogeography of man (Zeuner, 1945). This application of geomorphology, and, it must be admitted, a multitude of other techniques, is parallel and similar to its application to human geography or the geography of contemporary man.

Climate and Geomorphology

When one contemplates the relatively long Pleistocene period of fluctuating climate, about 1,000,000 years, and the relatively short period since the last glacial climate faded into a more genial climate, some 9000 years ago, he raises the question whether our land forms are products of the present climate or not. In the writings of Davis it is implicit that the land forms of each climate are the products of the existing climate projected into the past except for areas that were glaciated. Glacial forms are considered "climatic accidents" and by analogy even the forms of arid regions are so included, as is evident from a scanning of Cotton's book "Climatic Accidents in Landscape Making." Such an organization of geomorphology rests on tortuous reasoning. The "normal" climate of Davis, i.e. the temperate, humid climate of northeastern United States, northwestern Europe, and a few other places including parts of New Zealand, is truly exceptional. Most of the world enjoys or suffers from wholly different climates. Furthermore, in the Tertiary the distribution of climate was quite unlike that in the Pleistocene. Even the humid, temperate, or "normal" climate was largely displaced by a severe periglacial climate during each glaciation (Zeuner, 1945; Cailleux, 1942). Thus the interpretation of land forms rests more and more on paleoclimatology. Once well-developed from this viewpoint, geomorphology will itself become one of the tools in the refinement of paleoclimatology and lead to a more perfect history of the Earth.

Geomorphology Applied to Geography

If we follow the modern definition of geography that it is a science which deals with the distribution of man in respect to his environment, obviously geomorphology is not geography. Only from an historical viewpoint can geomorphology be considered a part of geography. Thus the ancient geography included many now separate sciences. The physical geography of Davis' youth has fallen apart into a complex of specialties: geodetics, topographic surveying, cartography, tidal and magnetic studies, physical oceanography, meteorology, climatology, hydrology, and so forth. These specialties enjoy an uneasy alliance as a part of geophysics. This blanket term combined with geochemistry includes also large parts of geology under the names vulcanology and tectonophysics. These alliances are based largely on the application of chemical and physical techniques to the age-old problems of geology and geomorphology. Useful as the alliances may be in research, they daily widen the gap between the "earth sciences" and geography.

If, then, we think of geography as at present practiced by the members of this Association, it is in the qualitative stage. The relations of man and his works to his environment are exceedingly complex. Our geographers are attacking their problems by observational techniques based largely on the methods of Natural History. Only in the field of demography are quantitative and statistical methods as yet in use. Many attempts to use statistical methods have met with resistance (Stewart, 1947; Zipf, 1949). On the other hand geology, somewhat reluctantly, to be sure, has embraced the quantitative and experimental methods of geochemistry and of

geophysics. Physical geography has almost completely gone over into this methodology. Geomorphology as yet is largely one of the Natural Sciences. However, the increasing influence of sedimentary techniques, the continued pressure for more precise results is gradually changing the picture. The paper in this symposium by Strahler reflects this new attitude as he has resorted to quantitative and statistical analysis. How otherwise can the effect of the pluvial-fluvial process on slopes be evaluated?

If modern geography is to be separated by methodology from a large part of physical geography and geomorphology, how is the geographer to gain the basic knowledge of environment? Must he also abandon the methods of Natural History and become a quantitative and statistical scientist? It is just possible that a large part of this quantitative development in geomorphology is for him beside the point. As Davis insisted, what is necessary is "the clear, the genetic description."

The sciences basic to modern geography must be applied to its needs. Geomorphology, now in a rapid state of development, is no exception. If it can be applied to other subjects it can be applied to the needs of geography. I propose now to discuss this application from two viewpoints—that of the geomorphologist and that of the human geographer.

Geographic Geomorphology

The general type of geomorphologic work which can be considered geographical was long since defined bv Davis and his contemporaries. Davis had no question that all his essays were geography. They provided the genetic analysis necessary for description. They provided a "clear and explanatory terminology." Yet he left to his contemporaries the definition of physiographic provinces and sections. The laborious compilation of descriptive matter lacked interest for him, yet the description of the United States is perhaps the greatest geomorphic achievement of his followers (Fenneman, 1931 and 1938). This work continues to go on. New physiographic boundaries have been proposed in Illinois by Leighton, Ekblaw, and Horberg (1948), in Washington by Freeman et al. (1945). Frye and Swineford (1949) have advocated revision in Kansas. New details have been discriminated in the San Luis Park of Colorado by Upson (1939). New descriptions of large areas have been written. Cooke's "Scenery of Florida; Interpreted by a Geologist" (1939), Schoewe's "Physiography of Kansas" (1949), Williams' "Volcanoes of Oregon" (1949), Bostock's "Physiography of the Canadian Cordillera, etc." (1948) are examples. Nor are our foreign colleagues less active; perhaps they are even more ambitious. King's "South African Scenery" (1942) and Hills' "Physiography of Victoria" (1940) are models of professional skill. Guilcher's (1948) detailed physiography of Brittany describes a diversified area with a minutiae unknown with us. Hol's "Geomorphology of the Netherlands" is a chapter in a handbook of its geography (1949) and has a clarity of illustration which we can only envy. It is true that all these books contain geologic terms; all refer to the remote geologic past. Thus all contain that "extraneous matter" contemptuously rejected by some of our

modern geographers. But they all conform to Davis' prescription in that they describe land forms in terms of their genesis. Over and over he insisted that a mere empiric description led to endless and easily forgotten repetition. These are new, up-to-date descriptions of the land in the best Davis tradition, produced, so far as I am aware, without any pressure from the geographic profession, by men no one of whom is a member of this Association. It is true that they are not addressed to the geographer but to an audience composed of the intelligent layman, the engineer and land planner, and to the inquiring geomorphologist. Where else can the geographer turn?

One must admit, as Russell (1949) has pointed out, that thus far too little attention has been paid to the great alluvial plains. On these plains are crowded some of our densest populations. However, many of these areas are far from present centers of geomorphic activity, yet the literature on the Ganges Plain, on the Hwang Ho, the Yangtse, and the Nile is very extensive. It is true that no one has put all this material together in easily accessible form. Perhaps all that is wanting is a demand on the part of geographers.

In fact, one wonders what application of geomorphology to geography is lacking? Every type of work which ingenuity can conceive has been done somewhere in the United States. The areal spread is not complete. We do not have complete physiographic descriptions of each state, but is there a strong demand? To my knowledge only in Wisconsin have serious local geographic studies been made in which the physiographic subdivisions of the state established by Martin (1916, 1932) have been systematically used.

If geomorphology is to be applied to geography, there should be a two-way decision on the type and kind of geomorphic work that is most useful. So far, the geomorphologists have, under Davis' leadership, made the decisions. The human geographer has been largely passive.

TRAINING OF GEOGRAPHERS

It would only add to existing confusion if one more voice were raised in the clamor as to the proper objectives of geography. Yet the confusion over objective leads to uncertainty in training. In a recent discussion of "Physical Geography in the Training of a Geographer" I have taken two positions which I believe are sound: (1) that physical geography as a unified subject no longer exists; it has been replaced by specialties, and (2) of these specialties climatology is by all odds the most important as the distribution of man is influenced more by climate than by topography.

Now the implication that geomorphology has a secondary rôle may not be in accord with our pride and would perhaps have been indignantly repudiated by Davis. It is, however, in accord with practice. A glance at the composition of departments of geography shows that most of them have specialists in climatology, many have cartographers, and a few maintain geomorphologists. Obviously, training in climatology and cartography is highly technical and involves many subjects

and facts extraneous to geography. Yet, students submit to this training because it is so obviously necessary. Nor do we hear complaints as to the separate trends of these subjects, although the needs of the human geographer are only a part of their objectives.

In general, geographers obtain their training in geology and geomorphology from the busy members of departments of geology. Obviously, the courses given are designed to teach the application of geomorphology to geological problems. The application to geography is neglected. Furthermore, instruction is designed to teach the student to use geomorphology as a tool in its geologic-geomorphic applications. Whereas from the standpoint of geography the intent should be to teach the student to understand geomorphology and to use the results of geomorphologic work.

Correction of this condition may be achieved in two ways. Geomorphologists can be appointed in departments of geography, with the implication that they will devote their whole time to teaching and research in the application of geomorphology to geography. Or a true cooperation with departments of geology should be attained. At the moment, geographers are so jubilant in being freed from geological interference that such cooperation may be difficult to achieve. Yet it is likely to be the more fruitful as it will be a cooperation between equals. A geomorphologist appointed in both departments would be a natural mediator and further, while keeping abreast of his subject for geological courses, would continuously distill the latest fruits of research for his geographic courses.

It is difficult to overemphasize the importance of the two divergent viewpoints outlined above. The human geographer needs to be and should be selective in the type of material that he uses. Unquestionably, environment is fundamental. How much of the enormous mass of accumulated scientific fact about environment does he need to use at any one moment? In all this aggregation of learning are the critical facts present? Perhaps physical geography is deficient in just that modicum of knowledge which is the crux of the human relationship that he seeks to understand. Has the human geographer been adequately trained as a skillful student and analyst of geomorphic data?

On the human geographer falls a responsibility. He must be sufficiently learned in all phases of physical geography, including geomorphology, to be a competent critic. He must be selective, use what he needs and reject what he can not use. The different categories of physical geography are careening across uncharted seas, each in pursuit of its own phantom ship. Now and then a sympathetic voyager drops a bit of intellectual pabulum for the unlearned. Occasional papers and books summarize subjects for the benefit of other scientists including the geographer. The competent geographer can not, however, depend on these charities. He must be sufficiently learned and industrious to follow the specialists and retrieve the essentials for an orderly and penetrating analysis of the elements of environment.

To William Morris Davis it would be shocking to learn that modern geographers complain that the history of land forms introduces matter extraneous to geography. To him the history of the landscape was essential to its understanding. Its under-

standing was a necessary prerequisite for forming that chain of association by which the repeating patterns of the earth could be held in memory. To me, his reasoning is sound. The empiric description of landscape is a monotonous repetition of hill and valley, river and brook, lake and swamp, one category separated from another merely by dimension. Davis advocated the visualization of ideal types of hills and valleys, each with a genetic name which by reason of its sound and meaning would act as an aid to the memory. He would have been delighted by the cartographic summaries of this type of genetic description produced by A. K. Lobeck and Erwin Raisz. Their geomorphic maps are beautiful examples of summaries of geomorphic facts and relations. But are maps and cartographic diagrams adequate? Must not the written description also be made? For the scholarly, the "clear, the explanatory description" will survive. Its function is logical and irreplaceable. Its usefulness is, however, depreciated in the ears of the ignorant or the intellectually slothful.

One may, quite properly, raise the question as to how much of this "explanatory description" is necessary at any one time or for any one geographic paper. Description of the earth in any of its aspects, geomorphic, climatological, or what not should not be introduced for the purpose of displaying the learning of the geographer. It should not be introduced just because a little scientific knowledge might be good for the reader. Direct quotation and copious footnotes used merely to convince the reader of the scientific verity of statements should be avoided. The skill of the well-trained geographer should guide him to abbreviation and to other literary devices by which the background of his study of the moment can be brought forward and yet prevented from confusing the issue. In such judgment and in such literary skill the geographer should be initiated by the schools of geography and by the example of his elders.

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Remarks by Guy-Harold Smith:

I have been greatly interested in the kind of training in the field of geomorphology which should be required of all professional geographers. It is clear today that a good many geographers for one reason or another are receiving very brief and perhaps inadequate instruction in this field. At The Ohio State University, where I have some responsibility in respect to graduate work in geography, I urge all of our students, at least those who are candidates for the Doctor's degree, to take a course on the systematic geomorphology and the physiography of the United States, both of which are taught in the Department of Geology. Occasionally, I hear objections on the part of some of the students, for they feel that the course is too geological in character and has only limited application in the field of geography. I do not give much attention to these objections, for the reason, that I feel that they are not well founded. The man who teaches this course is a New Englander, and he has brought to us on the west slope of the Appalachians some of the gospel which was originally heard in the hallowed hall of a New England University. I refer to Professor Richard P. Goldthwait, son of a distinguished New England geologist, who teaches these courses at The Ohio State University. In his course in geomorphology he has geologists, geographers, soil scientists, and plant ecologists, and he knows full well that his students come to him with different interests and perhaps even inadequate preparation, but I am sure that he understands clearly his educational responsibility to these various students. That geomorphology is at times biased in favor of the geologists would hardly be justification for omitting the subject in the training of a geographer. We hope at Tne Ohio State University that a sound and substantial course in geomorphology will continue to be available to our advanced students in geography. We would not insist that it be organized particularly for our students.

DAVIS' CONCEPTS OF SLOPE DEVELOPMENT VIEWED IN THE LIGHT OF RECENT QUANTITATIVE INVESTIGATIONS

ARTHUR N. STRAHLER

Columbia University

THE purpose of this paper is to review briefly the concepts of slope development held by W. M. Davis, to explain the contrast between Davis' explanatory-descriptive system of geomorphology and the dynamic-quantitative system endorsed by G. K. Gilbert, and to note certain uses of quantitative techniques in the study of slope problems.

We find in Davis' essay entitled "The Geographical Cycle" (1899) a rather complete discussion of his concept of the development of erosional slopes in the normal cycle in a humid-temperate climate. He states that the grading of slopes follows upon the grading of streams in the stage of late maturity of the region. He reviews the processes involved in slope wasting and notes that streams and hill-side waste sheets are only the extreme members of a continuous series in the drainage system. The waste sheets of a slope establish grade first at the base; this is extended up-slope to the divides. The concept of declining slopes is clearly expressed in his statement that "just as graded rivers slowly degrade their courses after the period of maximum load is past, so graded waste sheets adopt gentler and gentler slopes when the upper ledges are consumed and coarse waste is no longer plentifully shed to the valley sides below."

Davis' treatment of this and other geomorphic subjects was completely qualitative. I do not recall having seen a measurement of slope angle or a precisely measured slope profile in any of his publications. Neither is there any penetrating analysis of erosional processes based on mechanics of fluids or plastic materials, although his deductions seem to show an intuitive grasp of the dynamics. Davis' treatment appealed then, as it does now, to persons who have had little training in basic physical sciences, but who like scenery and outdoor life. As a cultural pursuit, Davis' method of analysis of landscapes is excellent: as a part of the basis for the understanding of human geography it is entirely adequate. As a branch of natural science it seems superficial and inadequate.

Consider now the quantitative-dynamic treatment of slopes and other landforms. This system has as its aim the expression of quantitative laws relating process to form. The landforms must be measured systematically if we are to understand the dimensions of the form-elements involved and to compare the magnitudes of these elements in various landscapes. At the same time the study of processes must be pressed forward in the field and laboratory in order to give laws of dynamics, quantitatively expressed. Only when the relationships between process and form are stated in quantitative terms is the goal of the quantitative-dynamic investigation

reached. Seen in this light, geomorphology is not a simple, pleasant nature-lover's hobby, but a geophysical science of almost terrifying complexity. To make substantial progress along these lines requires a type of training not ordinarily given in geology departments and perhaps wholly unobtainable in our geography departments. In addition to building a sound knowledge of geologic materials and topographic forms, the student must develop the necessary facility in higher mathematics and master the basic principles of fluid and plastic mechanics, thermodynamics, meteorology and hydrology, statistical methods, and a number of other subjects from the physical sciences. It is difficult enough to find geology students whose aptitudes and interests enable them to follow such a program; it seems almost out of the question that this program can be carried on by geographers.

This seeming digression is essential in forming a background for understanding the more recent trends in study of slopes and slope-forming processes. Actually, the quantitative-dynamic method of geomorphic study is as old as Davis' explanatory-descriptive system, for it was promoted strongly by G. K. Gilbert, whose classic report, "The Transportation of Debris by Running Water" (1914), greatly advanced our knowledge of stream activity. In the preface to this work Gilbert writes:

> "Thirty-five years ago the writer made a study of the work of streams in shaping the face of the land. The study included a qualitative and partly deductive investigation of the laws of transportation of debris by running water; and the limitations of such methods inspired a desire for quantitative data, such as could be obtained only by experimentation with determinate conditions. The gratification of this desire was long deferred but opportunity for experimentation finally came in connection with an investigation of problems occasioned by the overloading of certain California rivers with waste from hydraulic mines. The physical factors of those problems involve the transporting capacity of streams as controlled by various conditions. The experiments described in this report were thus instigated by the common needs of physiographic geology and hydraulic engineering."

Had Gilbert's philosophy of physical geology prevailed among students of landforms the analysis of slopes would not have been so long delayed. As it was, apparently Davis won over a large following and the study of landforms became dominated by such figures as Douglas Johnson, C. A. Cotton, N. M. Fenneman, and A. K. Lobeck, whose splendid contributions to descriptive and regional geomorphology have provided a sound base for studies in human geography.

Even as recently as 1943, Douglas Johnson presented his graduate classes in geomorphology with subject matter faithfully reproducing the principles and details as written by Davis 45 years earlier. An inevitable result of this state of affairs has been a gradual reduction in the physical science background of geomorphology students and teachers; and a consequent general sterility in original geomorphic research. But while academic geomorphology has been approaching stagnation important developments in the understanding of slope erosion processes have been made by hydrologists, hydraulic engineers, and soil erosion specialists concentrating upon soil conservation and sedimentation engineering. In the middle and late 1930's, largely under the broad program of research by the Soil Conservation Service important work was initiated, the published results of which soon began to appear.

As an example of the application of quantitative-dynamic methods to slopes, one may cite as typical the results presented at a symposium on the dynamics of land erosion held in Washington in 1941. Papers included: "Dynamics of water erosion on land-surfaces," by Leonard Schiff and Robert Yoder; "Sheet-erosion—past and present" by Robert Horton; "Dynamics of entrainment of erosional debris and sedimentation," by Carl B. Brown; "Physiographic engineering: land-erosion controls" by W. C. Lowdermilk; "A concept of the mechanics of the erosion-cycle" by Robert T. Knapp. In these investigations the engineering methods of research and analysis are stressed. While directed at erosion on a slope, treated as a unit area without much reference to the slope forms or the evolution of assemblages of landforms, these and many more similar investigations deal with essentially the same basic subject matter as that which the geomorphologist needs to treat in his study of natural slopes. What is particularly noteworthy here is that this line of progress did not stem from the work of Davis and his students, but arose quite independently from engineering sources. Obviously a generalized, over-all scheme of landscape evolution stated in terms of "youth," "maturity," and "old age" could contribute next to nothing to the understanding of factors determining the mechanism and intensities of erosion on slopes.

Encouraged by the Research Committee on Land Erosion of the American Geophysical Union, the analysis of erosion processes by engineering specialists continues to develop; but few geologists seem aware of this progress and there has been little evidence of geomorphologists adapting the information and methods to landform research. On the other hand, Robert E. Horton, a hydraulic engineer, attempted to relate the engineering knowledge to an understanding of landforms through a paper entitled "Erosional Development of Streams and their drainage basins; hydrophysical approach to quantitative morphology" which appeared in the Bulletin of the Geological Society of America in 1945. This paper is emerging now as a document of great importance in geomorphology, not so much for the validity of its conclusions on drainage development and slopes, as for the forceful manner in which it has brought to the attention of geomorphologists the application of quantitative and dynamic methods to landform study.

Following along some of the general lines suggested by Horton and others already referred to, the author has begun an investigation of the measurement and comparison of elements of form which comprise a fluvially dissected landscape. This includes not only the form, steepness, and length of erosional slopes, but the characteristics of the drainage basins and networks as well. The necessity of measuring actual topography in the field has required the use of standard statistical methods for analysis of sample data. The introduction of statistics is thus a new element not present in the work of Horton and other engineers concerned with slope erosion problems. The quantitative analysis of slopes is only the first step in the dynamic-quantitative method. It merely gives us information on the actual nature of slopes and their differences, but even this seems to have been sadly neglected by the followers of W. M. Davis.

Using a standardized sampling technique the writer has obtained data on the maximum angles of slopes present on the valley walls of maturely dissected masses behaving essentially as homogeneous rock materials.[1] Along with these measurements various other types of information have been secured. Applying the methods of frequency distribution analysis and representing these slope distributions by histograms certain points become evident. Slope means range very greatly from one region to another, but within a selected area of uniform conditions of climate, vegetation, bed rock and stage of development, slopes tend to cluster closely about a mean value. In each area, therefore, the slopes show a definite adjustment to the controlling factors. Moreover, most distributions seem to be arithmetically normal. Tests have failed to disclose significant departures from a normal distribution. Fitting the data of one of these areas, a region of dissected alluvial fan gravels near Bernalillo, New Mexico, to the normal curve of error a remarkably good fit is obtained.

If we take samples both of the slope angle of the valley walls and of the stream gradients at the base of the slopes the two sets of data show a good positive correlation. Plotted on log-log paper the points fall close to a straight line to which an estimating equation may be fitted. Interpreted in the light of Davis' concept of graded streams and slopes we might say that within a given area of mature topography, where structural controls are not evident, streams and slopes are part of a continuous graded system in which the gradients are best adapted to maintaining a steady state in the process of erosion and transportation of debris. The consistent decrease of ground slope accompanying decrease in stream gradient may be taken as an expression of Davis' concept of progressively reclining slopes as the stage advances. What we have done then, is to substitute quantitative statements for Davis' qualitative statements and this gives us knowledge which can be used in a study of the magnitude of causative factors.

Frequency distribution information on slopes lends itself to tests of the significance of differences in sample means. As an illustration, two slope samples were obtained from one small area of rugged, mature topography. One sample consisted entirely of slopes against the base of which streams are now actively corrading their channels, removing all debris derived from the slope. A second sample from within the same area consisted of slopes protected at the base by accumulations of slope wash or talus. The protected slopes, subject only to processes of rain wash and creep have a much lower mean angle than the slopes actively undercut at the base. This suggests strongly that slopes recline in angle with time, unless constantly refreshed by stream corrasion. Davis' concept of reclining slopes in the erosion cycle may thus be to some extent confirmed.

In conclusion, it might be added that the cycle-concept of Davis does not seem well adapted to expression of the dynamics of the erosion process. Instead, the

[1] For a detailed statement of the investigations described in this paper see: A. N. Strahler, (1950) "Equilibrium Theory of Erosional Slopes Approached Through Frequency Distribution Analysis," Amer. Jour. Sci., CCXLVIII, Oct. and Nov. (in press).

concept of a steady state in an open system seems a logical replacement for the idea of "maturity," while the stage of "old age" may well be abandoned. This change will bring the theory of the erosion-transportation process into line with systems of flow of fluids and heat, and various other dynamic systems which reach and maintain steady states.

Remarks by Louis O. Quam:

Efforts toward a more quantitative analysis of landforms is highly commendable and it is to be hoped that through further investigation of this sort our science may become more objective. Is there not a danger, however, that the use of mathematical formulae and statistical analysis give a false impression of objectivity to sample measurements the selection of which, in the field, is necessarily subjective? It has been my limited experience in slope measurement that very different results are obtained depending on the length of slope measured. Obviously, the personal element enters strongly in the selection of the sample measurements taken. Do we then, through exact mathematical analysis of selected samples, obtain the objectivity desired? Are we not in danger of giving an impression of accuracy which is not warranted?

THE GEOGRAPHIC CYCLE IN PERIGLACIAL REGIONS AS IT IS RELATED TO CLIMATIC GEOMORPHOLOGY

LOUIS C. PELTIER

Washington University

INTRODUCTION

DAVIS, in his early writings (1899–1909) recognized, by implication, that some aspects of the geographic cycle were related to the climatic regime under which they were formed. This was corollary to his classic formula: form is the result of the influence of structure plus process plus stage. Davis, in recognizing the existence of the arid and glacial cycles, which he called climatic accidents, also recognized that process was dependent upon climate.

This implication was examined in greater detail by various European writers, notably Albrecht Penck (1905, 1910), Passarge (1926) and Thorbecke (1927), and in America by Bryan (1940). Recently Büdel (1944, 1948) has suggested the recognition of "formkreisen" or "morphogenetic regions." If the geographic cycle were defined in terms of the climatic regime under which it develops, the minor forms of the landscape would become emphasized. Regional distinctions in topography which transcend structural regions might then become defined in our consciousness. Even changes in climate would become recognizable in the landscape.

It is not possible, with our present knowledge, to formulate precise definitions of the morphogenetic regions in terms of climate or the peculiar erosion cycle with which they are to be associated. One can only speak qualitatively and on the basis of personal impression. Nine different climatic regimes of geomorphic significance, and therefore nine different morphogenetic regions may, however, be tentatively postulated. Their recognition is based upon the supposed relative significance of the different geomorphic agents in a given climatic region. The climatic distinctions, here used, are crude and might be considerably refined. As, however, our knowledge of the relative importance of the geomorphic agents is even less precise, the climatic regimes are presented in terms of mean annual temperature and rainfall only (see Table 1).[1]

Seven of the morphogenetic regions roughly coincide with "geographic cycles" or "climatic accidents" previously described. Two other climatic regimes should also have geomorphic processes of differing character or intensity and should therefore produce characteristic geomorphic features. They are the "boreal" and "marine west coast" climates described by Köppen (1923) as the Dfc and Cfb climates respectively.

That the processes of intensive frost-action in the periglacial areas constitute a cycle has only recently been recognized by Bryan (1946) and Troll (1948). As

[1] This form of climatic diagram resembles those used by Blumenstock and Thornthwaite (1941) and also many of their predecessors.

TABLE 1

MORPHOGENETIC REGIONS

Morphogenetic Region	Estimated Range of Average Annual Temperature	Estimated Range of Average Annual Rainfall	Morphologic Characteristics
Glacial	0°–20°	0″–45″	glacial erosion nivation wind action
Periglacial	5°–30°	5″–55″	strong mass movement moderate to strong wind action weak effect of running water
Boreal	15°–38°	10″–60″	moderate frost action moderate to slight wind action moderate effect of running water
Maritime	35°–70°	50″–75″	strong mass movement moderate to strong action of running water
Selva	60°–85°	55″–90″	strong mass movement slight effect of slope wash no wind action
Moderate	38°–85°	35″–60″	maximum effect of running water moderate mass movement frost action slight in colder part of the region no significant wind action except on coasts
Savanna	10°–85°	25″–50″	strong to weak action of running water moderate wind action
Semi-Arid	35°–85°	10″–25″	strong wind action moderate to strong action of running water
Arid	55°–85°	0″–15″	strong wind action slight action of running water and mass movement

this cycle is current only in remote portions of the arctic and in high mountains, but was of great importance in the periglacial areas of temperate regions during the Pleistocene, it is here called the periglacial cycle following Troll (1948). In Davis' terminology the periglacial cycle is a "climatic accident." If, however, the regime of the periglacial cycle be considered in comparison with that of the other cycles, including the moderate cycle ("normal" cycle of Davis), it may be found to have equally persistent characteristics. The cycle must then be supposed to have equal validity with other cycles.

Each cycle is here considered to be "normal" within its own regime and a "climatic accident" only when it temporarily encroaches upon the area of another regime. Any climatic regime may produce both a normal cycle and a climatic accident, depending upon the fluctuations of climate that may have occurred in the past. For these reasons the name "moderate cycle," which implies origin under a climate of

moderate temperatures and rainfall, is used, here, for the cycle of moderately humid, temperate regions, in place of "normal cycle." Lands which fall within the zone of climatic fluctuations may have the peculiar characteristics of one cycle superimposed upon those of another. A composite product of the two regimes, here called a polygenetic topography, would then result. In a consideration of the effect of human activity on erosive process and in the analysis and description of the landscape the concepts of morphogenetic regions and polygenetic topographies may be useful.

THE GEOGRAPHIC CYCLE

Davis' Concept

The concept of a geographic cycle which Davis presented in 1899 is the concept of an ideal cycle logically deduced from an assumed set of circumstances. Its importance lay in the expanded picture which it gave to the geomorphic landscape, previously interpreted largely in terms of lithology, geologic structure and geologic process. Davis stressed the importance of time and presented the concepts of a recognizable stage of development in the topography and of the genetic classification of landforms.

His writings (1894, 1899) indicate that he considered structure and stage to be the most important elements in the development of landforms, for he recognized a classification of landforms in terms of each. Lithology appears to have been considered as a minor element represented in the topographic expression of structure. Process was assumed to be that which prevailed under the ideal or "normal" climate and was therefore considered to be essentially constant for most purposes. Variant or "abnormal" climates of arid and frigid regions were recognized and the temporary invasions of these climates into the realm of "normal" climates were treated as accidents. They were considered to be of minor importance in the general trend toward peneplanation (1894).

In his recognition of "abnormal" climates as different from "normal" climates and as productive of different erosional regimes, Davis laid the foundation for an additional, climatic, classification of landforms. The "normal" climate was defined as one in which the precipitation, chiefly in the form of rain, is sufficient to fill all basins to overflowing; the arid climate as one in which wind replaces water as the chief agent of transportation; and the glacial climate as one in which ice is the chief agent of transportation. This geomorphic classification is therefore based upon the relative importance of the various geomorphic processes, which are, in turn, related to climatic regimes. It may be further expanded and refined by a consideration of the climates and the various geomorphic processes which become active under them.

Morphogenetic Regions

If this line of investigation be followed in the direction indicated by Albrecht Penck (1905, 1910), Hettner (1921), Passarge (1926), Thorbecke and others (1927), Bryan (1940), Blumenstock and Thornthwaite (1941), Büdel (1944, 1948),

Troll (1948) and others, a series of climatic regimes may be established within which the intensity and relative significance of the various geomorphic processes are, according to our present information, essentially uniform. These regions are morphogenetic regions. They serve to further define and describe the geographic cycle.

A distribution of morphogenetic regions, with respect to either climate or space, may be postulated from a consideration of the realms of activity of the various geomorphic processes. These realms may be further subdivided by considering whether the process is merely present, or is dominant, or is intermediate. These hypothetical realms and the morphogenetic regions postulated therefrom are presented below on graphs in figures 1 to 6. Because, as has already been explained, accurate means of measuring and comparing these processes are not available, these graphs represent merely a diagrammatic exposition of a concept and are necessarily a reflection of the author's reading and thought. Inevitably the ideas of others have been used and the author here acknowledges his debt to his predecessors whose writings have inspired him.

The different morphogenetic elements consist of the processes of rock weathering and the transportation of these products. The processes of rock weathering consist of chemical decomposition and mechanical disintegration. Together they account for the major part of the material made available for transportation. The ratio of their effectiveness to the effectiveness of transportation determines the presence or absence of exposed rock and the thickness of residual soils.

Chemical decomposition, which consists primarily of the oxidation, hydration and solution of the various mineral constituents of the rocks, may be theoretically related to the climate. Rainfall, insofar as it determines the availability of water for chemical reactions, and temperature, as it determines the speed of chemical reactions—these are essential elements. All of the processes of chemical decomposition require water either as a constituent in the reaction or as a solvent and transporting agent for the products of the reaction. The presence or absence of water is broadly synonymous with the presence or absence of chemical decomposition. If it is assumed that the availability of water varies directly with the rainfall, then, other conditions being assumed equal, the probable rate of chemical decomposition will vary directly with rainfall. There is, moreover, a tendency for the rate of chemical reactions to increase as temperature increases (Arrhenius, 1889).[2] This tendency toward direct variation of chemical weathering with temperature is augmented in humid lands by the increased density of vegetation, with increase in both rainfall and temperature, leading thereby to an increased production of organic acids in the soil. Chemical reactions which are related to organic wastes are, therefore, most rapid in the warm, humid regions.

The relations set forth above form a crude but effective measure of the relation of chemical decomposition of rocks to climate. In figure 1, the maximum rate of chemical decomposition is placed in the humid tropics and there are shown two zones

[2] This tendency may, however, be offset by a distinct decrease of solubility of oxygen and carbon dioxide in water with increased temperature (Glasstone, 1940).

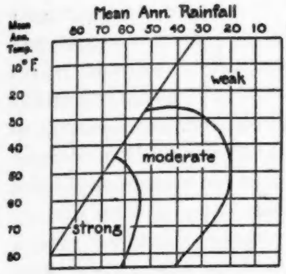
of minimum rate of decomposition, one in the low-latitude deserts and the other in the cold, high-latitude deserts.

Mechanical disintegration, where it is divorced from agents of transportation so as to exclude abrasion and corrasion, probably is largely frost-produced. The idea that mechanical weathering or the break-up of rocks by temperature changes is characteristic of desert areas has largely been abandoned under the attack by Blackwelder (1925). For this reason frost action is taken, in the following discussion, to represent the overwhelmingly greater part of mechanical disintegration. As has already been amply shown by Steche (1933) and others, the intensity of frost action is dependent upon the frequency of temperature fluctuations about the freezing point in the presence of water. There are, thus, two thermally-controlled minima of frost action, where the temperatures are too warm for freezing and too cold for thawing. The final picture of the distribution of frost action is derived by superposing the rainfall pattern with its sub-tropical and arctic minima upon the thermally-controlled pattern. The result, shown in figure 2, indicates that there is a maximum zone of frost action located in the relatively humid sub-arctic regions. Frost action decreases in intensity not only in the direction of the warmer and drier regions, but also toward the colder and drier areas of the Far North.

The distributions of chemical decomposition and of mechanical disintegration, as represented by frost action, shown in figures 1 and 2, may be combined to produce a generalized representation of the distribution of weathering as shown in figure 3. This generalization indicates that there are seven major categories or regions in which weathering processes take characteristic forms. As these processes produce soils they may be considered also as pedogenetic regions. Within these pedogenetic regions no one of the weathering processes occurs exclusively. In each region the different processes occur together in unique proportions.

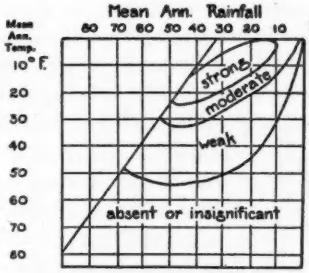
Rapid chemical weathering prevails in the humid tropics, rapid mechanical weathering in the humid sub-arctic. In the arid regions the rate of weathering of any kind is relatively slight. Moderately rapid chemical decomposition probably prevails in the semi-arid low latitudes, and slow or relatively insignificant frost action may be anticipated in the semi-arid sub-arctic regions. Both chemical and mechanical weathering are, in this diagram, assumed to be present in the humid, mid-latitude regions. If the rate of erosion and erosional history are assumed to be uniform and constant, the deepest residual soils should occur in the humid tropics and the humid sub-arctic, and the shallowest residual soils in the mid-latitude and high-latitude deserts.

The comminuted material provided by these weathering processes is subject to erosion by one or more of three agents of transportation. These are running water, mass movement and wind. The action of waves of the sea is here excluded from consideration, for its activity, although universal in all climates, is limited in area to the coasts. Waves cannot have the same significance in the development of morphogenetic regions as do the other three agents of transportation. The geographic cycle of wave action or marine cycle (Johnson, 1919) has, however, a certain validity.



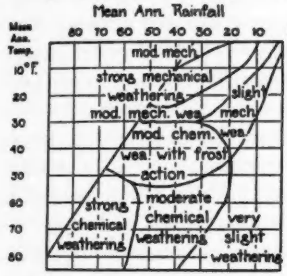
Chemical Weathering

FIG. 1.



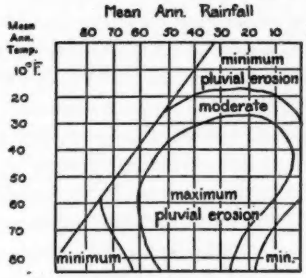
Frost Action

FIG. 2.



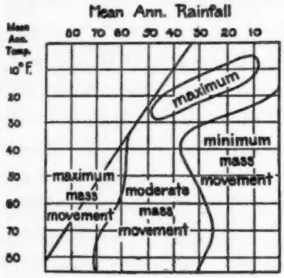
Weathering Regions

FIG. 3.



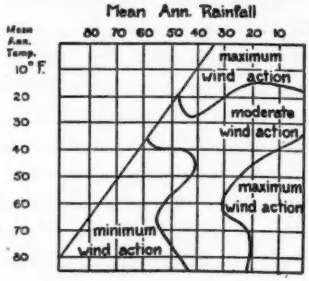
Pluvial Erosion

FIG. 4.



Mass Movement

FIG. 5.



Wind Action

FIG. 6.

Its local peculiarities and possible subdivisions related to the climatic characteristics of storminess, wind variability and the biologic elements of the growth of grasses, forests or corals lie beyond the scope of Johnson's marine cycle. When the marine cycle has been considered from a climatic standpoint it may possibly be recognized in relation to polygenetic topographies.

The importance of running water as a geomorphic agent was emphasized by Davis. Within the regions in which he made his observations it is certainly the most effective of the agents. Running water, as it is considered here, includes slope wash and rill wash as well as the flow of both clear and turbid streams. This agent cannot be everywhere equally important in every landscape, for its action is dependent upon five main variables: 1) the intensity of the precipitation, 2) the frequency of storms, 3) the permeability of the ground, 4) the rate of evaporation and transpi-

ration, and 5) the nature of the plant cover. Also, in order to permit the maximum effectiveness of fluvial erosion, the rate of weathering must equal or exceed the rate of removal by water. Under these circumstances barren rock would never be exposed and the running water would always be flowing over comminuted and readily transported material. The effectiveness of running water as an erosive agent is further limited by the vegetation. Wherever the plant cover is sparse or only moderately dense the vegetation acts to inhibit gully erosion, but does not prevent slope wash and rill erosion. On the other hand, wherever the plant cover is dense and the mantle of fallen leaves lies thickly upon the floor of the forest, the leaves act as a roof to prevent the infiltration of rain water into the ground. The water tends to run off on the surface of the fallen leaves and both slope wash and gullying may nearly cease. This situation is marked in the tropics and was observed by the writer in 1944 and 1945 on the flanks of the northern end of the Cykloop Range near Hollandia and Tanah Merah in New Guinea.

Considering the foregoing all too brief resumé of the principles of erosion, three areas of minimum effectiveness or speed of erosion by running water may be outlined. These, shown below in figure 4, are in: 1) the mid-latitude desert regions of low rainfall, 2) the arctic and sub-arctic regions of low rainfall, and 3) the tropical regions of heavy plant cover. The realm of maximum pluvial erosion is similarly shown to be located in a climate intermediate in character between those of the three erosional minima.

Mass movement, as an erosive agent, is here taken to include landsliding, mud flowage, creep, congeliturbation and solifluction insofar as one differs from the other. All forms of mass movement are produced by the saturation of thick deposits of loose material, sometimes aided by frost action, with the possible exception of dry creep.

The word "creep" as defined by Sharpe (1938) is so broadly applied as to indicate only the existence of minor or conditional instability and the gradual subsidence of the slope. It cannot be used to relate a process to a climatic regime. Thus creep under frost conditions is congeliturbation; under moist conditions, soil flowage; under dry conditions it constitutes a particular form of displacement brought about by thermal expansion and contraction. No separate term exists for this latter process. It is thus suggested that the word creep be restricted to the process of downslope movement of particles by expansion and contraction through temperature changes in the dry state. Creep may then become a characteristic feature of arid regimes, as contrasted with periglacial and humid regimes.

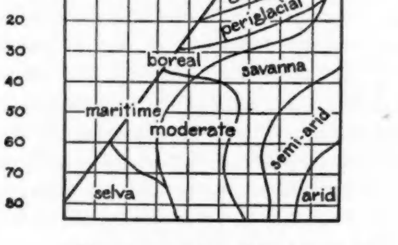
Two broad categories of mass movement may indeed be recognized, based upon the presence or absence of freezing. The requisite conditions for this process are: 1) a rate of weathering which exceeds the rate of erosion (denudation), and 2) an accumulation of soil moisture. The most likely places for the occurrence of strong, or statistically rapid, mass movement are, thus, in those parts of the regions of rapid chemical and mechanical weathering, illustrated in figure 3, in which denudation from slopes by sheet wash and gullying is least effective. Two areas of maximum intensity of mass movement, shown in figure 5, may therefore

be anticipated, one in the humid tropics (as described by Sapper, 1935), one in the region of marine west coast climate, and another in the periglacial area of intense frost shattering, solifluction and congeliturbation (Kessler, 1925; Steche, 1933; Büdel, 1937; Troll, 1944). Mass movement should be least effective, or statistically slowest, in regions where both the rate of weathering and the available soil moisture are least. These are the arid and semi-arid regions. Occasional spectacular landslides and mud flows do occur in these regions (as described by Alter, 1926). Many of the landslide scars and deposits of arid regions, however, are ancient and were formed during the moister periods of the Pleistocene (Hoots, 1930; Smith, 1936; Reiche, 1937).

Wind action is probably the slowest and least effective of the erosive agents (see Cotton, 1942). This may be true because deflation is limited to grains of the smallest diameter and because deflation does not tend to be as narrowly concentrated at a point or line as are the other, previously considered, agents. Furthermore, minor topographic irregularities and the irregular surfaces of trees and shrubs produce a sufficient roughness to protect the surface of the ground from wind scour. Because of its relatively slight effectiveness, wind action may be apparent only where the effective action of running water and mass movement is slight; because a plant cover reduces surface wind velocities and inhibits deflation, wind action may be apparent only in barren regions; because topographic roughness also inhibits wind velocities, strong wind action should be limited to relatively broad, open country. In a broadly regional sense, excluding topographic influences as well as the influences of coastlines, two areas of maximum effectiveness of wind action may be postulated, one in the warm, dry, mid-latitude deserts and the other in the cold, dry, circumpolar deserts. It is, however, unlikely, except in instances of extreme aridity, that wind action should predominate over the other geomorphic agents, even in these places. The area of least effective wind action is probably that of the humid tropics where the encroachment of swamp vegetation onto the shoreline prevents even the development of coastal dunes. This distribution of wind action is illustrated below in figure 6.

Nine different climatic and possible morphogenetic regimes, illustrated graphically in figure 7, may be postulated from the foregoing analysis. Each should be distinguished by a characteristic assemblage of geomorphic processes. Thus, if cycles are to be defined in terms of the agents and regimes which produce them, they should correspond to nine different geographic cycles. Davis has admitted the existence of the moderate cycle (called by him "normal") (1899), the arid cycle (1905), and the glacial cycle (1909). Each of these "cycles" can easily be identified with one of these climatic, morphogenetic regimes. Cotton has recognized a savanna cycle and a semi-arid cycle (1942) which, in a climatic sense, are intermediate between the arid and moderate ("normal" of Davis) cycles. He also mentions briefly a "hot-humid" cycle based on observations by Sapper (1935) and Freise (1938). In order to maintain brevity of name, and in keeping with Cotton's use of the ecological term

"savanna" to describe characteristic processes of the "inselberg landscape" of Bornhardt (1900), this "hot-humid" cycle is referred to as the "selva cycle." This term is appropriate because the protective effects of the high tropical forest, or selva, prevent or inhibit erosion on hill slopes by running water either as slope wash or in gullies. A thick soil, vulnerable to mass movement, is therefore able to develop (see Wentworth, 1943) and the characteristic geomorphic features of this cycle may thereby be formed. The most recently described separate regime is called the periglacial cycle by Troll (1948) and includes the cycle of cryoplanation of Bryan (1946). It will be discussed in more detail below. Thus, of the nine climatic re-



Morphogenetic Regions

FIG. 7.

gimes, and corresponding cycles, here postulated, seven have already been described as producing unique geomorphic results. For the remaining two regimes, here called the maritime and boreal regimes, no distinct geomorphic characteristics have so far been reported. The probable erosional features of these as well as the other regimes are presented in table 1. Possibly the unique properties of the boreal regime have been obscured by the relatively recent prevalence of the periglacial cycle. Its morphogenetic characteristics may not be independently discernible. The maritime regime, though in many places recently subject to the effects of the glacial and periglacial cycles, may be recognizable by the severity of mass movement. The limiting climatic boundaries of these regimes are shown graphically in figure 7. The climatic boundaries of this graph are in part the same as those given by Penck (1910), Davis (1912) and Troll (1947) whose influence is acknowledged by the writer. Particularly are the glacial, selva and arid regimes of figure 7 parallel to the glacial, humid and arid climates of both Davis and Troll. I should, however, say that many of the ideas here expressed are explicitly stated in the lectures of Professor Kirk Bryan, whose emphasis on climatic morphogeny has led to the formulation here set forth.

If the geographic cycle is considered to be the sequence of events leading to the complete destruction of the geomorphic landscape, irrespective of the details of topo-

graphic form which may be produced or the details of the geomorphic processes which are effective, there can be but one geographic cycle. From such a broad viewpoint the other "cycles" here defined are unnecessary. Because the run-off of water from the land surface is, over a long period of time, likely to be the dominant agent in molding the surface, this single cycle is best described as the "pluvio-fluvial cycle." However, if the development in youth and maturity of minor topographic forms, particularly the development of slopes, is to be stressed, then the pluvio-fluvial cycle is an inadequate framework for the description of surface features peculiar to the several morphogenetic regions. Each geographic cycle is here considered as the unique expression of a climatic regime. Each acts through successive stages upon rocks of differing lithologies which are themselves arranged in various structures. Landforms are therefore to be described by an expanded Davisian system: 1) structure including lithology, 2) process as modified in nine morphogenetic, climatic regimes, and 3) stage.

THE PERIGLACIAL CYCLE

One of these cycles, the periglacial cycle, has been described and its properties established by many workers whose reports have appeared throughout the past fifty years. Notable among these are Matthes, Anderson, von Lozinski, Högbom, Cairnes and Bryan. Matthes (1900), while studying the geomorphology of the Big Horn Mountains, recognized *nivation* as a distinct geomorphic process. In 1907, Anderson described the movement of rubble on the slopes of the Falkland Islands. He described the process of movement in terms of frost action and saturation and named it *solifluction.* Von Lozinski (1909), as a result of his studies of weathering in the Carpathian Mountains, recognized the former existence of a peculiar climate, which he called a *periglacial climate.* This climate was influenced by the proximity of the Pleistocene ice sheets. It existed in the regions peripheral to the continental ice and was characterized by intense frost action. Cairnes (1912-A, 1912-B) recognized a peculiar process of down-wastage active in the mountains of Alaska. This process, which he called *equiplanation,* resembles in some respects the *"peneplanation by weathering"* of de Terra (1940). Similar phenomena were studied elsewhere in Alaska by Eakin (1914, 1916) and interpreted to be the result of a special kind of solifluction process which he called *altiplanation.* Högbom (1914, 1926) called attention to the spectacular frost phenomena in Spitzbergen and Scandinavia and postulated therefrom a mechanism of *frost-heaving.* The concept of frost action and frost-produced erosion has recently been both unified and enlarged into the concept of *cryoplanation* by Bryan (1946). He considers that, under a frost climate, hillslopes are reduced by a process of denudation or down-wastage by frost action as described by Cairnes, Eakin and others. The products of frost action are removed by rivers which flow only in the summer season of melting as described by Poser (1936). Cryoplanation is thus comparable and parallel to the peneplanation of temperate regions. From the foregoing it may be concluded that the concept of a

peculiar periglacial cycle is not new, but has gradually evolved over a period of fifty years.

The periglacial cycle, in its ideal sense, is illustrated in figure 8. This series of diagrams shows how a hypothetical hill and valley of a periglacial region might be modified by the continued action of geomorphic processes in the proportions peculiar to this regime. Following the pattern set by Davis (1899) three stages, early, intermediate and penultimate, are described as youth, maturity and old age.

At the outset of the cycle, during the youthful stage (figure 8-B), the slopes are attacked by frost-shattering (i.e. congelifraction) and are both steep and jagged. Joint patterns must play a strong part in the detailed configuration of such rock slopes. Wherever the joints are widely spaced cliffs may predominate and a talus of coarse angular débris may accumulate at the base; wherever the joints are closely spaced the slopes may be more gentle and the talus may more closely resemble a scree of fine fragments (Behre, 1933) which is the rock-fragment or shale-fragment slope of Judson (1949) and Peltier (1949). This talus may, in the presence of adequate moisture, be subjected to movement and erosion by freezing and thawing as has been described by Capps (1910) and Russell (1933). As the talus grows it provides a lower limit for the attack of weathering upon the exposed rock of the slopes and a continuous slope begins to develop which, in the upper portions, is formed by a congeliturbate mantle lying upon rock, and in the lower portion consists of congeliturbate lying over and incorporating the talus. The beveled, congeliturbate-covered rock surfaces in the upper part of these slopes are the early phases in the development of a surface of cryoplanation. These slopes may have gradients of about 15 to 20 degrees. At first they are neither wide nor long. They are separated, one from the other, by steep-sided rock valleys with slopes of 25 to 30 degrees (or even steeper), such as were described from Spitzbergen and Greenland by Poser (1936). In places distinct, steep-sided, angular or sub-angular bodies of rock may rise abruptly from the gentle congeliturbate slope as though partially submerged in a sea of frost-produced fragments. Such steep-sided rocks have been illustrated and described by Cairnes (1912-B) and Eakin (1916). Undissected remnants of earlier cycles or previous geologic events may remain in the more remote parts of the hills. Here congelifraction (defined by Bryan, 1946), without the accompanying horizontal displacement of congeliturbation, may be expected. Its depth of development would be controlled by the depth of the zone of ground water saturation and by the depth of freezing and thawing and, therefore, by the extent of its exposure. The solid rock surface should, thus, assume a gently rounded profile. The stage of youth in the periglacial cycle should be characterized by:

1) gently rounded, congelifractate-covered, undissected upland remnants,
2) jagged, frost-riven cliffs at the base of which a talus of angular, frost-shattered fragments has formed,
3) gently sloping, congeliturbate-mantled, steep-sided, re-entrant valleys in the cliffs whose slope is continuous with the top of the talus,

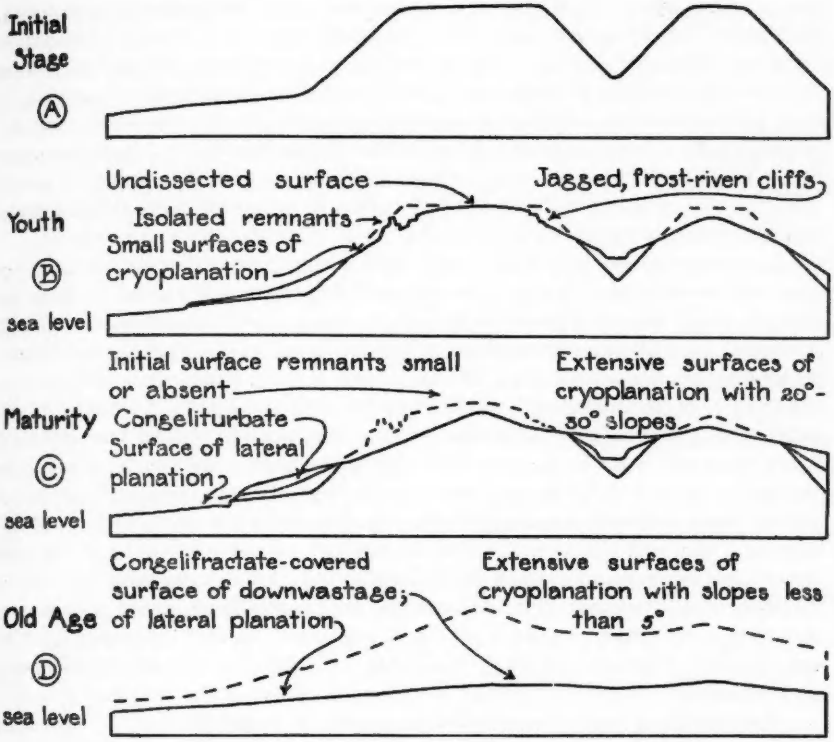


FIG. 8. The periglacial cycle.

4) isolated, steep-sided, frost-riven remnants surrounded by the gently sloping, congelifractate-mantled surface of cryoplanation.

The stage of maturity (figure 8-C) is marked by the disappearance of the isolated rock remnants, the frost-riven cliffs and the broad, gently rounded, undissected upland, and by the extension of broad surfaces of cryoplanation. This change is brought about by the retreat of the slopes bordering the small surfaces of cryoplanation developed during youth and as the result of destruction by congelifraction of all exposed rock where water is present and freezing-and-thawing frequent. The slope retreat is the result of denudation by congeliturbation, soil-flowage and the slope wash effected by thaw waters and rainstorms. Except in those places which are continuously and effectively subject to river or wave erosion, the products of this denudation will accumulate at the foot of the slope. Wherever this accumulation develops and grows it establishes a gradually rising local base level which limits the denudation. As a result of this process the congeliturbate-mantled slopes, under

the periglacial cycle, may be expected to retreat by successively decreasing gradients. The frost-riven cliffs of the youthful stage, on the other hand, probably retain their steepness as they retreat until they are ultimately consumed as the gradually rising slope of cryoplanation is extended. The extension of surfaces of cryoplanation is most actively carried on by congelifraction concentrated at a horizon within the ground where water is abundant and where the fluctuations of soil temperature about the freezing point are numerous. Thus, wherever the ground water table occurs within a zone of frequent alternations of freezing-and-thawing, very severe congelifraction should occur.

In shallow valleys where the water table lies near or at the surface, this may result in the development of a surface accumulation of congelifractate. Beneath the hilltops, where the water table lies at greater depth below the surface, the zone or horizon of most intensive congelifraction may lie beneath the surface, buried by less-thoroughly fragmented material. Its magnitude and significance are determined by the effect of thermal insulation of the overlying rock and by the fluctuations of the water table. The congelifractate thus formed may move gradually down the slope under the action of the same processes which produced it. At the point of inflection between steep and gentle slopes, where in warmer climates springs might be expected, this concentrated congelifraction may undermine the slope, cause it to collapse as a slice and lead to its parallel retreat. This process of mechanical ground water weathering, determined by the contour of the ground water table, will lead to the progressive lowering of the hilltops and the progressively increasing radius of curvature of the hilltop profiles in periglacial regions. The surfaces of cryoplanation are therefore developed by both weathering (congelifraction) and erosion (congeliturbation).

The broad surfaces of cryoplanation, developed during maturity, are assumed to be smooth and unbroken by sharp or distinct valleys. Their gradient is relatively steep, varying between 20 to 30 degrees on the upper slopes and less than 5 degrees near the valley bottoms. Because frost action, including congelifraction, acted over a longer period of time, in producing a mature stage, than was required for the youthful stage, the fragments in the congeliturbate should also have been reduced to finer and smaller sizes than had previously prevailed. The mature stage of the periglacial cycle should, then, be characterized by:

1) long, smooth, gently sloping, undissected, congeliturbate-mantled slopes,
2) broadly rounded hill-tops and hill-crests,
3) broad, gently sloping, congelifractate-covered valleys or broad, flat valleys filled with congeliturbate and coarse alluvium derived therefrom,
4) the absence of cliffs and remnants of the previous cycle.

The old-age stage of the periglacial cycle (figure 8-D) is the product of continued congelifraction and congeliturbation acting upon the gently sloping surfaces and rounded ridges of the mature stage. Two features characterize this stage. The continued mechanical ground water weathering and the continued downhill move-

ment of congeliturbate have led to the destruction of the hills and the reduction of the slopes to gradients of less than 5 degrees. The congeliturbate, by this time, must also be thoroughly comminuted by congelifraction. The sand- and silt-sized particles produced by this process (Zeuner, 1945) are vulnerable to wind action. Loess and sand deposits on the one hand and wind-swept pebble-pavements on the other may, therefore, be important during this stage, for the relief is now so reduced as not to seriously interfere with the wind, and plant cover may only locally protect the surface.

The dominant agents of planation active in the periglacial cycle, frost action and wind action, are theoretically independent of ultimate base level. Wind action is, relatively, so ineffective that the possibility of a complete cycle due to the wind need hardly be considered. The products of frost action are, however, significant and must be removed, or equiplanation as postulated by Cairnes (1912) would be universal. The work of streams in removing the waste of frost action determines the local base level. The periglacial cycle may, therefore, develop and go to completion either about a through-flowing drainage or about closed basins. However, should ultimate old age be attained, it must be related to sea level. Such a plain must, then, resemble in kind, if not extent and coarseness of surface material, those of Lehtovaara and Skansberget in northern Sweden described by Högbom (1926).

EVIDENCE OF A PERIGLACIAL CYCLE

Evidence of a periglacial cycle is widespread throughout the sub-arctic and high mountain regions. To these areas may be added regions in which a periglacial regime prevailed during parts of Pleistocene time. Most of the existing information relates to characteristic deposits, as relatively little attention has been given to the erosion surfaces which are generally concealed by these deposits. The concept of planation by frost action was first mentioned by Wright (1910). He considered that it might occur above the limit of glacial ice and thus explain some topographic features he observed in Iceland. He did not, however, particularize on the process. Cairnes (1912-A, 1912-B) described a surface of periglacial erosion (cryoplanation) in Alaska as an example of a process called equiplanation. He, however, focused his attention upon the cut-and-fill action of the process rather than upon its frost-influenced nature. The name equiplanation therefore became applicable to any region of interior drainage, even though he described it as characteristic of the arctic. Eakin (1914, 1916) added his observations of the process he called altiplanation. He described it as a special phase of solifluction that, under certain conditions, expresses itself in terrace-like forms and flattened summits and passes that are essentially accumulations of loose rock materials. This concept was applied to the interpretation of the top of Mount Washington, New Hampshire, by Antevs (1932), who argued that frost planation had produced the lawns, benches and spurs of the Presidential Range. It was further amplified by de Terra (1940), who described the process of cryoplanation in detail as applied to the tendency toward "peneplanation by weathering" of the Tibetan Plateau.

The effect of periglacial processes in the widening of valleys and the retreat of slopes has been observed in the Falkland Islands by Anderson (1907), in Scandinavia by Högbom (1926), and in Spitzbergen and Greenland by Poser (1936). Their studies show that the existence of saturated ground within the zone of freezing and thawing will lead to the development of a horizon of intense congelifraction and the development of a flat, rubble-covered surface. This surface may extend itself laterally by the action of frost at the base of the valley walls and the undermining of those walls. These surfaces are therefore analogous to surfaces of lateral planation of the moderate (or "normal") cycle as described by D. W. Johnson (1931), and the valleys may be so widened as to assume the appearance of maturely eroded river valleys. However, during the spring "break-up," the great rivers of the north also cut laterally much like rivers of temperate lands and hence accomplish "lateral planation." At the same time rock fragments torn from their parent rock by frost-shattering may, either by frost-heaving and subsidence or by plastic flowage of the surficial thawed zone over the permanently frozen subsoil, move down the slopes toward the center of the valley. This process smooths and extends the slopes and decreases their gradients. Its effects are analogous to those of slope wash under a pluvio-fluvial regime. The twofold nature of peneplanation, comprising denudation and lateral planation, which is apparent in the moderate cycle, is also present in the cryoplanation of the periglacial cycle. It may be comparable to the distinction, made by de Noë and de Margerie (1888), between slopes produced by rainwash on hillsides and slopes produced by rivers and streams.

Periglacial Features in Pennsylvania and Missouri

Features which represent late youth or early maturity in the periglacial cycle occur in the Appalachian Mountains of Pennsylvania and the St. Francois Mountains of Missouri. Here may be seen rock surfaces which must be interpreted as the result of cryoplanation, mountain crests which were lowered by congelifraction, broad, smooth, undissected mountain slopes shaped by congeliturbation and valleys widened by the laterally-extending action of frost (Peltier, 1949).

The destruction of the tops and crests of the Appalachian Mountains is illustrated by exposures in road cuts across the tops of Big and Little Mountains near Catawissa, Pennsylvania (Peltier, 1949). Here the development of angular fragments of sandstone appears to have lowered the elevation of the bedrock surface and permitted the accumulation of fragments of all sizes at the surface. These fragments, in one instance, show a sorting similar to that of the arctic cellular soils as though frost-heaving, as well as frost-shattering, had been important. On the slopes, at either side, these sorted bands are drawn downhill as a trail. The effect was to lower the elevation of the mountaintop and increase the radius of curvature of its profile, to fill in irregularities in the bedrock surface producing a smooth slope, and to bring about an accumulation of rubble toward the foot of the mountain, causing a decrease in the gradient of the mountain slope, as illustrated in figure 8.

The frost-molded landscape is not as prominent in Missouri as it is in Penn-

sylvania. This is, in part, due to the tendency of many of the rocks to break up into small fragments, in part due to the presence of thick deposits of loess which obscure the rock surfaces near the Mississippi River, and possibly also in part to a Pleistocene climate which was different from that of the Appalachian Mountains of central Pennsylvania. The frost features of the St. Francois Mountains of Missouri most closely resemble those of the Piedmont of southeastern Pennsylvania and Maryland.

The evidence of periglacial frost action in Missouri consists of boulder-strewn slopes, rubble deposits, rounded hilltops, smooth undissected or slightly dissected slopes, periglacial gravel terraces along the rivers, and poorly developed blockfields. To these features may be added the weathering characteristics of sandstones and limestones, such as the development of wide fissures and of a surface zone of angular fragments, which appear to resemble features described in Europe by von Lozinski (1909), Kessler (1927) and Cailleux (1943) and attributed by them to periglacial frost action.

The rubble deposits on the hillsides have the typical lack of sorting and typical angularity of fragments characteristic of congeliturbates. These angular fragments have not, except near the surface, been attacked by chemical weathering. Their corners are not rounded. In some places, where the loess is relatively thin, loess and congelifractate have moved together down the slope, accumulating about the base as a body of silty congeliturbate or as alternating layers of congeliturbate and disturbed loess, while the hilltop has either been left barren or veneered by a thin mantle of rock fragments and boulders.

The surface of the congeliturbate, in loess-free regions, is represented by boulder-strewn slopes derived from the congeliturbate by the selective erosion of fine constituents, and is analogous to that described by Shaw (1929) and Lowdermilk and Sundling (1950) as *erosion pavement*. The lower parts of these slopes are generally smooth and gentle, cut by scattered shallow gullies one to two feet in depth. The upper parts often interfinger between low cliffs and barren bedrock residuals which rise above the boulder- or stone-strewn slope. The slope may in some places extend beyond this zone to merge with the rounded top of the hill. Minor variations in this picture are the result of different rocks and different arrangements of rocks in the hills. These and other periglacial features of Missouri will form the subject of a later paper.

The present smooth and rounded hilltops are not in adjustment with the existing climatic regime. In a hill consisting of rocks which are vulnerable to the leaching and chemical disintegration of a humid regime the depth of weathering, countered by the rate of soil erosion, should be adjusted to the water table. Chemical disintegration should, because of the greater availability of both water and of reagents carried downward from the surface of the ground, be most active at a horizon at or near the ground water table. Such a horizon of chemical ground water weathering was observed by the writer in 1945 near Parang, Mindanao, Philippine Islands, where decomposition is interpreted to be proceeding at a greater rate in the moist soil and rock a few feet below the surface of the ground than at the surface. Similarly, as

shown above, the depth of optimum mechanical ground water weathering should be adjusted to the water table. Two types of ground water weathering thus exist and in each the weathered material should be thickest beneath hilltops. Certainly then, under conditions in which weathering dominated over erosion, the bedrock contour should not essentially coincide with that of the surface. There are, however, many places in which bedrock lies within a few inches of the surface of the uppermost parts of the hillslope and the hilltop. This must be interpreted either as the result of contemporary erosion which continually keeps pace with the rate of weathering or as a relic of a time in the past, when the rate of erosion was excessive and exceeded the rate of weathering, from which recovery has not yet been made. If one assumes that the erosive agent was pluvio-fluvial action, some difficulty is encountered in explaining how an adequate discharge of surface water capable of the necessary soil erosion could be concentrated on the hilltops. If, however, frost conditions are considered, erosion by congeliturbation and soil flowage presents no such difficulties. These erosive agents act throughout the thawed zone on gradients as low as two or three degrees. For this reason, and also because there is other evidence indicating the former existence of strong frost action, it is supposed that the rounded, soil-poor, rock hills were denuded during a cold episode of the Pleistocene, and that insufficient time has since elapsed to permit the redevelopment of a thick soil.

Missouri was subjected to the influence of the periglacial cycle, though to a less intensive degree than central Pennsylvania. It provides a picture of periglacial frost phenomena in a region which probably lay near the southern limits of action of that regime. Since the end of Pleistocene time this regime has changed from that of the periglacial cycle toward that of the moderate cycle, whereby the effects of the latter have been superimposed upon those of the former. The resulting topography is polygenetic topography.

POLYGENETIC TOPOGRAPHY

During the past century the existence of climatic changes, not only during the Pleistocene, but also within historic times, has become well-established (Penck, 1905; Köppen and Wegener, 1924; Brooks, 1949). This was recognized in the writings of Davis (1894, 1899, 1900-A, 1900-B, 1905, 1909) who considered that with the progress of the erosion cycle there were not only changes in orographic climates, but also that there had been temporary invasions of arid and glacial conditions into the regions ordinarily characterized by his "normal cycle." He called these geomorphic superpositions "climatic accidents" a terminology retained by Cotton (1942) in his textbook.

However, if, as suggested earlier in this discussion, the geographic cycle is to be interpreted in terms of the peculiar climatic regime under which it was formed, the moderate cycle becomes only one of several distinct and equally important regimes. Between these regimes there may occur transition phases and areas of the invasion of first one and then another climatic regime as the climatic zones have shifted or changed their properties. Wherever these changes are so recent that the effects of

the previous geographic cycle have not been erased, polygenetic topographies result. These topographies are the result of two or more superimposed morphogenetic regimes.

Such changes have already been recognized in weathering products and their effects described as polygenetic soils (Bryan and Albritton, 1943). They are equally apparent in the development of periglacial river terraces, as described by Soergel (1924), for here the valley-fill is produced under one regime and the erosion of that fill to produce a terrace under a quite different regime. A periglacial terrace may be regarded as the result of the superposition of the moderate cycle upon the periglacial cycle. Equally impressive is the evidence, found in both northern Missouri and northern Pennsylvania, of the dissection of the glacial deposits and glacially molded topography by the stream action of the present. Here, there is a superposition of the moderate cycle upon the glacial cycle. In many instances, particularly in areas unglaciated since early Wisconsin time, the periglacial cycle has been superposed upon the glacial cycle and has, in turn, been superposed upon by the moderate cycle.

The concept of polygenetic topography may be applied to the interpretation of slopes in both Pennsylvania and Missouri. This is possible because a slope which is formed, and which is stable under one regime, differs from the slope of another regime and is unstable under the different conditions of climate and plant distribution (see Lowdermilk, 1934). The slopes produced under a periglacial regime, by the dominant activity of frost action, tend to have gentle gradients, to be nearly straight or slightly convex upward in their middle section and to have broadly rounded tops. On the other hand, under a pluvio-fluvial regime, the action of running water requires that the water be accumulated into a stream to achieve its maximum erosive power. Because the water accumulates with distance down the slope, erosion tends to increase toward the foot of the slope. This tendency is counteracted by the increase in stream load and by the accumulation of alluvium at the foot of the slope. The resultant curve is concave upward. This curve is characteristic of slopes developed under the dominant influence of running water.

In a region, such as Missouri, which has been subjected to both regimes, a composite product, such as that shown in figure 9, must result. Here it is supposed that the slope was modified, from the moderate cycle, by the periglacial cycle, which caused an increase in the radius of curvature of the hilltop, straightened, lengthened and smoothed the hillsides, and filled up the valleys. It is then supposed that the slopes became stabilized by the encroachment of a cover of natural vegetation so that this relic topography was retained, even after a change back to the climate of the moderate cycle, and in fact until historic times. Exposure of this slope by deforestation and cultivation, however, has exposed the hills of Missouri to the unimpeded pluvio-fluvial activity of the moderate cycle. Gullies with a concave-upward profile have developed in the smooth, straight slopes of the periglacial regime. In this instance the contribution of man has been only to tip the scales slightly toward soil erosion and in favor of the existing climatic processes. The latent instabililty of the

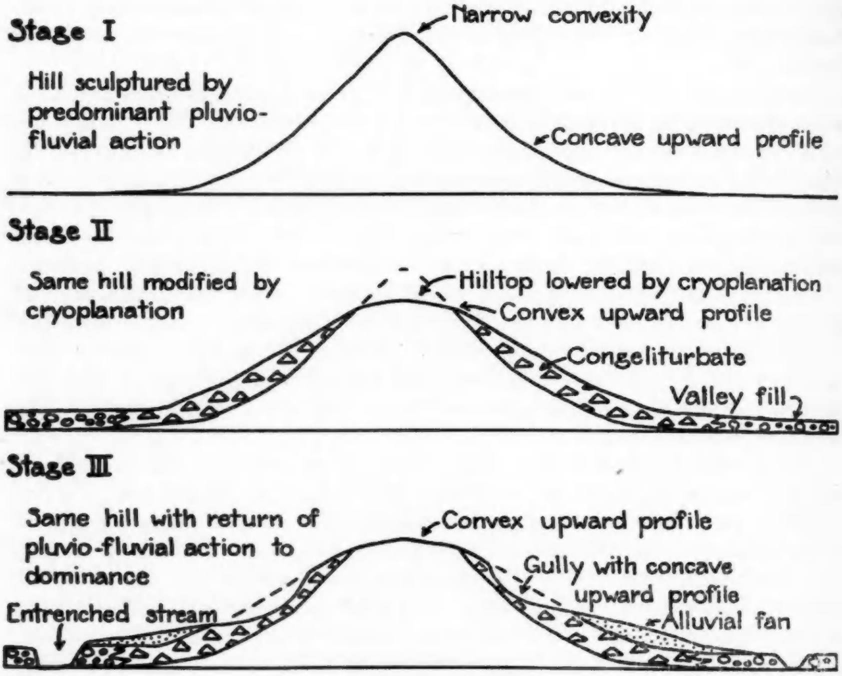


FIG. 9. Polygenetic topography.

slope under the present climate has led to gullying and all the other consequences known as "soil erosion."

SUMMARY

Davis, by his recognition that different geomorphic processes prevail under different climatic regimes, laid the basis for an analysis of landforms with respect to climate. This is inherent in Davis' classical formula for landforms (1899): landform is equal to structure, process and stage (time). "Process" may here be replaced by "regime" which represents the sum of all geomorphic processes, in those peculiar proportions, which characterize a particular climatic region. If a geographic cycle is defined in terms of its own regime, it may also be defined in terms of its climate.

A particular climate will produce a peculiar assemblage of geomorphic processes. It will therefore have its own peculiar geomorphic manifestations. Within an area in which the climate is essentially homogeneous, the geomorphic effects of climate may be assumed to be similar. These areas are here called morphogenetic regions.

Nine morphogenetic regions, with associated geographic cycles, are recognized or postulated:

1) glacial
2) periglacial
3) boreal
4) maritime
5) selva
6) moderate ("normal")
7) savanna
8) semi-arid
9) arid

All of these have climates and geomorphic processes which are "normal" for the region in which they occur. It is therefore misleading to call one normal and the others accidental. For this reason the name "moderate," which implies a regime of moderate temperature and moderate rainfall, has been substituted for the adjective "normal" as applied by Davis (1899) to a geographic cycle.

One of these cycles is the periglacial cycle. It occurs in the cold, humid subarctic regions wherein may be produced three coexisting types of erosion surfaces:

1) a surface of down-wastage or denudation produced by congeliturbation,
2) a surface of lateral planation, produced by the concentrated action of congelifraction wherever the water table and the zone of frequent freezing and thawing coincided, and aided by congeliturbation,
3) stream-graded surfaces, frequently aggraded.

If it may now be assumed that there is a peculiar assemblage and a peculiar proportion of geomorphic processes for each morphogenetic regime producing unique geomorphic features, then a change in climate may be expected to result in a change in the geomorphic details of the area involved. The result will be a polygenetic topography, such as occurs in Pennsylvania and Missouri. This concept is useful not only for the description of topography, but in analyzing its latent instability. It may thus be related to the interpretation of soil erosion.

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Remarks by Richard J. Lougee:

Some of the outcrops and rocky features in Pennsylvania and Missouri cited by Professor Peltier as illustrating late-Pleistocene periglacial action may be open to question, and perhaps be explainable as products of recent weathering. Criteria for periglacial action at this stage of our knowledge are still theoretical. However, some form of cycle as Professor Peltier has postulated is certainly expectable, and should be evident in regions that were recently subject to arctic or alpine cold. The characteristics of the cycle might differ with structure, topographic relief, slope, and other factors; and surely they would vary in intensity with latitude, altitude, and humidity. Without water in the soil it is unlikely that any periglacial action could take place. Hence it is to be expected that humid regions of great cold are the best places to seek evidence to work out the stages of the periglacial cycle.

Before the regions of old activity are investigated it would seem advisable to examine and evaluate the criteria in regions now actively subject to periglacial processes. Such a place is the humid and cold mountain country north of Nome, Alaska, where all slopes are subject to movement. (Lantern slides shown.) Solifluction terraces on these slopes appear to be moving like thin frosting running off a cake; their fronts which may be six feet high are in the process of rolling forward over the tundra vegetation growing on the next lower terraces. Since that time in the Ice Age when the Nome Mountains were smoothed by ice cap glaciation, this post-Glacial mass movement of solifluction terraces has been so great that residual towerlike spines of bedrock, or "tors," have been left projecting through and as much as thirty feet above the active layer on summits and upper slopes of the mountains. Despite the smoothness in appearance of the Nome Mountains the sheet erosion and removal of bedrock has been not less in amount than the full height of the tors.

Criteria more or less like those at Nome may be looked for in regions of former periglacial action. For example, Devonshire, in southwest England, was never glaciated, but presumably it always has been a humid region like Nome, and quite possibly may contain vestiges of old periglacial action. Tors on hilltops are a well known feature of the scenery in Devonshire, though not usually explained as a phenomenon of the periglacial cycle. Southwest England would seem to merit Professor Peltier's attention after criteria in regions of active periglacial action have become thoroughly understood.

MANUFACTURING IN THE ROCK RIVER VALLEY—LOCATION FACTORS

JOHN W. ALEXANDER

University of Wisconsin

IN the valley of the Rock River which flows through southern Wisconsin and northern Illinois is one of the many small manufacturing areas which are located on the fringe of the main United States Manufacturing Belt. Within that main belt are large and well-known industrial areas such as Chicago-Gary, Detroit, Cleveland, Pittsburgh-Youngstown, Buffalo, Philadelphia, New York, and Boston. All of these have one or more of several advantages: cheap water transportation, proximity to raw materials, local power resources, or nearby markets. But on the margin of this manufacturing belt are several small industrial districts (Figure 1) which do

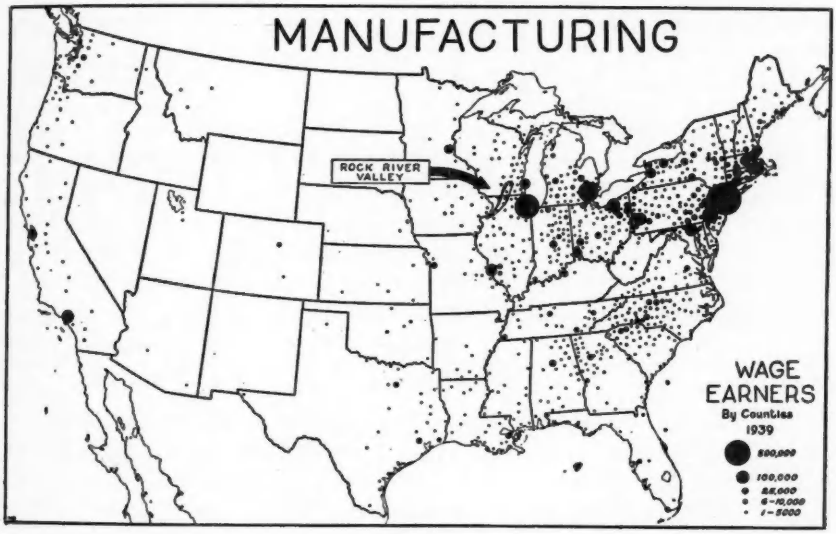

FIG. 1.

not possess these advantages and whose development therefore poses the question: What are the factors which account for the location of manufacturing in smaller cities fringing the main manufacturing belt? The Rock River Valley is one of these small industrialized areas and is here taken as the case study in considering this question.

The pattern of industry in the Rock River Valley resembles a prong projecting down the valley into the agricultural Midwest. There are 93,000 people employed by factories in this valley, and 56% of them work in Rockford, Beloit, and Janes-

ville. These three cities, located within a 28-mile strip of territory, constitute the industrial core of the valley. Although they are not large communities, their degree of industrialization is high (Table I).

TABLE I

	Population	Employees in Manufacturing	Percentage of Population
Rock River Valley Cities[1]			
Rockford	110,000	35,000	32%
Beloit	35,000	10,600	30%
Janesville	26,000	5,800	22%
Selected Metropolitan Districts[2]			
Akron, Ohio	423,000[2]	106,000[3]	25%
Detroit, Michigan	2,702,000	611,000	23%
Youngstown, Ohio	380,000	87,000	23%
Pittsburgh, Pennsylvania	2,100,000	350,000	17%

[1] Statistics based on field work data, 1948.
[2] Bureau of Census, Current Population Reports, Series P-21 Number 35, *Population Characteristics of Metropolitan Districts, April 1947.*
[3] ———, Current Population Reports, Series P-51 Number 35, *Labor Force Characteristics of Metropolitan Districts, April 1947.*

Secondary metal fabricating is the leading type of manufacturing in Rockford, Beloit, and Janesville. Machinery, hardware, machine tools, and automotive equipment are the most important products (Table II).

TABLE II
EMPLOYEES BY INDUSTRIES
ROCKFORD, BELOIT, AND JANESVILLE
1948

	Number		Percentage	
Metal Products	40,000		78%	
machinery		12,600		24%
hardware		8,300		16%
machine tools		7,600		15%
automotive		7,000		14%
foundries, forges		1,500		3%
miscellaneous		3,000		6%
Furniture and Wood Products	3,300		6%	
Textiles	2,000		4%	
Foods	1,500		3%	
Leather and products	1,500		3%	
Miscellaneous	3,100		6%	
TOTAL	51,400		100%	

Industrial development in the Rock River Valley has occurred even though the region lacks assets which most manufacturing regions possess. It has only small reserves of *raw materials* other than the products of dairy farms and gravel pits. Most of the raw material is iron and steel which comes in a variety of forms from

Chicago, Detroit, Pittsburgh, and other eastern sources. There is no significant *power* resource at hand. No coal is mined in the valley. The only power produced indigenously is hydro-electric and accounts for only 2% of the power generated in the region. Furthermore, there is no substantial encircling *market*. The major market is to the east, in the main United States Manufacturing Belt, the very region from which come most of the raw materials. Lastly, the Valley is not blessed with any particular advantage in *transportation facilities*. The Rock River is not and never has been used for much commercial transportation. A few small dams without locks prevent navigation, but more important is the orientation of the river. It flows from north to south whereas the traffic into and out of the region is in an east-west direction. The Rock Valley does have adequate railroad transportation, but it does not have any better service than numerous Illinois or Wisconsin cities which have just as good railway connections, but much less manufacturing.

Why then has manufacturing developed in the Rock River Valley? What are the factors which have encouraged industry to be located in a region which apparently has such a low endowment in raw materials, power, markets, and transportation advantages? It may be that the factors which explain the location of manufacturing here can explain the development of manufacturing in numerous similar locations on the fringe of the main United States Manufacturing Belt.

The explanation of the Rock Valley's industrial complexion involves five groups of factors. The first group is of historical importance and is inoperative today. The other four groups in general are currently effective. These groups of factors are: an early start when factories were established to serve a local market using local power and local raw materials; the human factor of an industrially inclined people; location in terms of transportation costs; advantages inherent in small cities; and miscellaneous factors.

A. AN EARLY START

Manufacturing began in the Rock Valley shortly after the region was settled in the late 1830's. The first settlers were farmers, yet soon after their arrival, there appeared two industries—saw milling and grist milling—the two phases of manufacturing typical of most newly-opened areas. In these early days the Rock Valley offered several bases for industry, and as early as 1860 was beginning to stand out as an industrial zone (Figure 2). The bases for Rock Valley manufacturing were local markets (which focused on cities along the Rock River), local water power, and local raw materials.

1. *Local Markets*

The influx of people into the Rock Valley between 1840 and 1860 produced a market for such essentials as food, housing, clothing, and agricultural implements. This market was based fundamentally on the agricultural possibilities in the Rock Valley. However there were other areas in the Midwest with just as good

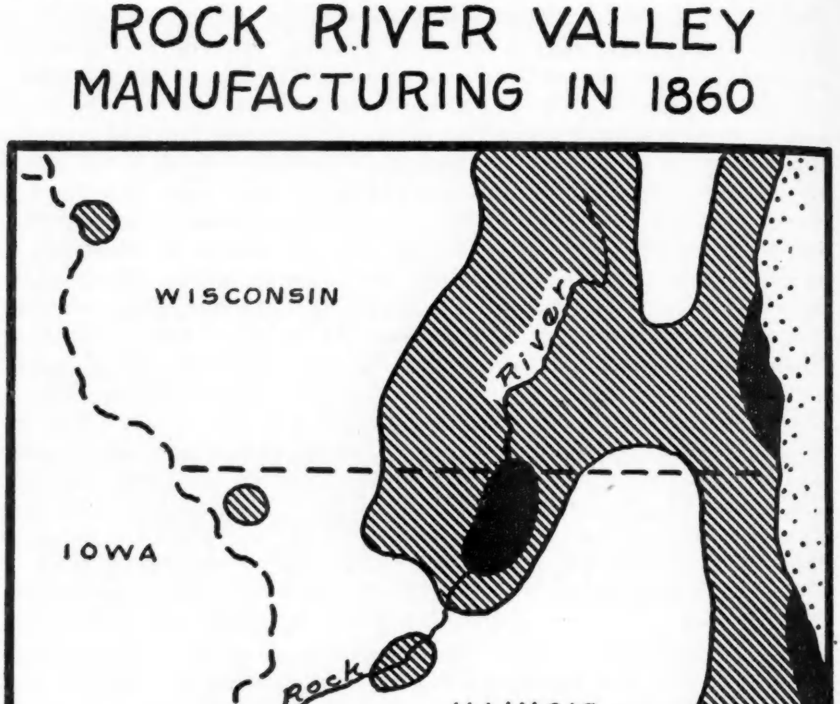
ROCK RIVER VALLEY
MANUFACTURING IN 1860
WISCONSIN
River
IOWA
Rock
ILLINOIS
Employees in Manufacturing
Per Cent of Population, by Counties
2%
1%

FIG. 2.

agricultural possibilities attracting thousands of settlers; but manufacturing has not thrived in them as it has in the Rock Valley. There were other factors operating in the Rock Valley. The most important of these was the Rock River itself which was effective in two ways: attracting settlement (thus localizing along its course a substantial portion of the market mentioned above), and providing water power.

Settlement along the river was encouraged because the stream was an obstacle to land transportation. The paths of migration were across the river, not along it. Where these paths found good fords or ferries, there developed settlements which became the largest in the general region. Hence the largest local markets were along the river, and industries sprang up in harmony with that pattern.

2. *Local Water Power*

The Rock River attracted industries to itself by offering water power sites. The first extensive development of the river's power was in Rockford where a dam was built across the rapids in 1843. Countless small streams in the Midwest originally had dams, but more industry developed along the Rock River because it was large enough to provide power for several factories. Yet its valley was not so wide as to make dam construction difficult. Larger valleys such as those of the Illinois, Wisconsin, and Mississippi rivers are not as industrialized as the Rock Valley; one reason is that they have wide floodplains across which it was difficult to construct dams a century ago.

Waterpower sites were the seed beds of manufacturing in the Rock Valley. Today the water power is an historical relic in most Rock River cities, because for years it has been more economical for factories to purchase power from public utilities than to generate their own.

3. *Local Raw Materials*

The first two industries in the Rock River Valley were based on the raw materials of lumber and wheat. Stands of walnut constituted the raw material on which Rockford's early furniture industry was based. This local supply of raw material was depleted before the turn of the century, and for years Rock Valley woodworking factories have been hauling raw materials from increasing distances, as far as Canada and the Appalachians. Although the factor of local timber is largely absent today, it did contribute to the initial establishment of factories which continued to operate even after the timber disappeared. The reservoir of skilled labor which had developed in using this timber remained as a stimulus to manufacturing.

Wheat was the Rock Valley's major crop for many years, and flour milling was a leading industry. But as better wheat lands were opened in the west, Valley farmers shifted to dairying, and flour milling disappeared as rapidly as did wheat growing. Water power which had been developed to turn the wheels of grist mills became available for other factories.

B. THE HUMAN FACTOR

Some industries are oriented on power; some are oriented on fuel; others are oriented on raw materials; and many are oriented on markets. But there are others that are *foot-loose-and-fancy-free* to locate wherever the leadership and labor supply are found.[1] Rock Valley industries in the main are in this category and are based to a high degree on the Valley's human element.

This human factor is very difficult to measure, in fact it is impossible to present tangible evidence for many of the points which will be made in the following discussion. Nevertheless, it is contended that the factor of *people* has played a fourfold role in the development of Rock Valley industry: industrial genius, productive labor, working for wages below those paid to eastern competitors, and willingness to supply capital.

1. *Industrial Genius*

Industrial genius has been manifest in two ways: the ability to invent and the ability to organize. It is impossible to list all the Rock Valley citizens whose inventions have stimulated the Valley's industry, but a few can be mentioned.[2] Rockford's most famous inventor was John H. Manny who invented the Manny reaper. By the 1850's Rockford had earned the name of "The Reaper City." Marquis L. Gorham invented three agricultural machines—the Gorham seeder, a corn cultivator, and a binder. William W. Burson also invented several agricultural machines, particularly the automatic grinding harvester. John Nelson, a furniture man, experimented with mechanisms in an entirely different field—knitting—and invented a machine which resulted in the birth of a new industry in Rockford. The effect of this invention will be discussed in more detail later. Rockford's rise to fame in furniture production was stimulated in 1882 by Robert Bauch's design of a combination book case and writing desk, a style which swept the country. For twenty years it was Rockford's main product with from twenty to thirty furniture factories specializing in its production. Previously Rockford's furniture market had been restricted to a small area including northern Illinois, southern Wisconsin, and northwestern Iowa. In the production of machine tools, the importance of Rockford can be traced to the inventions of W. F. and John Barnes who invented several types of saws, drills, lathes, and other tools. Elmer Woodward invented a governor for water wheels, and today the Woodward governor plant is the world's largest factory devoted exclusively to the production of governors. Howard D. Colman, a young inventive genius from Beaver Dam, Wisconsin began toying with ideas for textile machinery and developed the *hand knotter* for splicing two threads together with a knot. His inventions followed by about thirty years those of Nelson in the textile field and, like his predecessor, Colman started by thinking about textile machinery as a hobby. His warp-tying machine is probably his major invention.

[1] For a discussion of some of the theory behind the location of these foot-loose industries see E. M. Hoover, *The Location of Economic Activity*, p. 36.

[2] The author expresses appreciation for information about Rockford inventions to Mr. Edgar D. Lilja and Mrs. Elizabeth Hall of Barber-Colman Company.

He and his inventions were instigating elements in the origin of Rockford's second largest factory, which produces textile machinery.

Beloit also has had several inhabitants who conceived new ideas for machines and methods. John F. Appleby designed the first twine binder. Dorr E. Felt invented the comptometer which is universally used in business offices. L. H. Wheeler invented a device which improved the old style windmill and became the basis for the Eclipse Wind Machine Company, forerunner of Fairbanks Morse Company in Beloit. A. P. Warner invented the Warner speedometer employing the magnetic principle for measuring automobile speeds. This is still the basic principle in the operation of all such speedometers. F. N. Gardner invented a disc grinder, an invention on which two Beloit factories have developed. Carl E. Lipman's inventions in mechanical refrigeration and air conditioning were basic to their development.

Janesville's major invention was George Parker's fountain pen, and today the Parker Pen Company in Janesville produces about a third of the fountain pens made in this country.

This study does not purport to be an inventory of Rock Valley inventors. However, a few have been cited to support the statement that the human element in the Rock Valley has been effective through its inventive genius in hastening the advance of industry.[3]

The textile industry in Rockford is a good illustration of the manner in which the entirety of a single industry can stem from a single invention. In the 1860's John Nelson, a Swedish furniture maker who liked to tinker with machinery, visited an exposition in Chicago and saw a Lamb knitting machine, a manually operated mechanism which knitted men's cotton socks. Nelson got the idea of making an automatic machine to do the job. He sold his wood shop, secured some financial backing from a Rockford farm machinery manufacturer named Ralph Emerson, and began work on a knitting machine. By 1870 a machine had been perfected to knit socks automatically at a cost of two cents per dozen pair. The socks were seamless and received nationwide acclaim as "The Old Rockford Sock." That same year the Nelson Knitting Company was organized with more financial encouragement from Mr. Emerson. Today there are five other textile mills in Rockford and all are descendants of the Nelson Company. Were it not for John Nelson's invention, the textile industry probably would not be present in Rockford today.

The outstanding example of a genius for organization was P. A. Peterson, the most successful business executive in Rockford history. For over fifty years, from 1876 to 1927, this man's industrial genius was significant to the growth of manufacturing in Rockford. It was in the furniture business that he first gained recognition.

[3] A question might be raised here as to which came first, the industrial development or the inventions; that is, did inventors appear on the scene because this was a manufacturing region, or have the inventions produced the industry? Actually, this is a chicken-and-egg type of question. Certainly invention was stimulated by existing industry, but it is just as certain that Rock Valley industry today would be far behind its present pace if the inventions had not been made. Manufacturing stimulated invention, and invention stimulated manufacturing. Inventions have been a definite factor in the Rock Valley's industrial growth.

Many of the first furniture factories in Rockford were cooperative ventures involving Swedish people. When these "co-ops" had financial troubles, they went to P. A. Peterson who, as a result of his business successes, had acquired a substantial fortune; but more than that, he had acquired such a reputation that a note with his signature would be honored by Rockford banks. Consequently "P.A.," as he is referred to now, wrote several personal notes to help distraught furniture plants. Frequently a plant could not pay him when the note came due; he then accepted shares of stock in payment. Subsequently at the next meeting of the board of directors he would be elected president. This happened so many times that "P.A." became identified with almost every furniture factory in Rockford and was referred to as "The Furniture King of America." Ensuing successes brought prosperity to the companies and a fortune to him. After becoming firmly established in the furniture enterprise he began to lend helping hands to other businesses. At the time of his death in June, 1927 he was involved in 50 enterprises and was president of nine. One cannot delve very far into the story of Rockford industry without running across his name.[4] An analysis of Rockford's industrial expansion which omitted the role of P. A. Peterson would be just as incomplete as one which omitted the role of water power. P. A. Peterson's performance is the outstanding example of the role of industrial directive ability in the Rock Valley.

2. *Productive Labor*

The Rock Valley has a supply of productive skilled labor which constitutes an important element underlying its industrial development.[5] When it comes to appraising this labor factor one encounters a nebulous element which defies measurement in tangible forms. It is possible to measure an industrial region's physical resources such as coal, water power, and raw materials. One can measure transportation costs of assembling these essentials and of shipping the finished products to market. But measuring the labor element, patricularly its quality and productivity, is extremely difficult. It is especially difficult to compare different regions in terms of this element. This is the crux of the problem in analyzing the Rock Valley Industrial Region, for the element basic to the growth of manufacturing there is an element which has never been measured in terms as concrete as those measuring other factors. In spite of this difficulty, an effort is made to show that ***productive skilled labor*** is the Rock Valley's major resource for manufacturing.

In 1947 a Chicago company was experiencing difficulty in finding productive workers, its experienced labor having been pulled away by other industries that could pay higher wages. Replacement labor was so unproductive that it could not even meet the company's minimum production quotas. This company moved to Rockford where, within a few weeks, the new labor force was producing at a rate which exceeded minimum quotas and qualified 90% of the employees for a bonus.

[4] Today when one inquires about how a Rockford industry got started he is likely to receive a reply beginning, "Well, there used to be a man around here named P. A. Peterson. . . ."

[5] For a discussion of the theory involved in location of industry in relation to the labor factor see E. M. Hoover, *op. cit.*, Chapter 7, "Location and the Labor Market."

In the case of this company, labor was an "initial location factor," attracting an industry to the community. In the case of every company which is thriving, the labor supply is a "survival location factor," enabling them to thrive in competition with similar companies in other regions.

Why and how did a quantity of labor accumulate in the Rock Valley? Initially there was a concentration of people there for reasons which already have been mentioned.[6] Furthermore, this concentration of population was most pronounced in Rockford for several particular reasons. Rockford had a rock bottom ford. It was on the direct overland route from Chicago to Galena where the lead mines supported the largest cluster of population to the west of Chicago. In 1852 the first railroad into the Rock Valley (the Chicago & Galena Union) reached the east side of the Rock River in Rockford, and for several years this was the end of the line.

Why has the labor pool been of high quality? One reason is that the people with talents in manufacturing developed skills and handed these down to their children. Thus, a reservoir of productive skilled labor accumulated through the years.

The outstanding example of this factor is in Rockford where people of Swedish descent have been the most conspicuous element in the increasing supply of labor.[7] Industry had a slow start in Rockford, but it grew rapidly after the arrival of the Swedes. The original Yankee settlers built the dams, sawmills, grist mills, and farm implement factories; John H. Manny invented his reaper before the Swedes arrived. Then came the first influx of people from Sweden to Rockford—in 1852. Many had known a trade in Sweden; a few had been cabinet makers in the old country. One of them, Andrew E. Johnson, established, on the water power, a small cabinet shop which developed into Rockford's first furniture factory. Rockford's Swedes quickly took to furniture making. New factories were built; by 1885, Rockford's reputation as a furniture center had spread over the nation. In 1893 Rockford had twenty furniture factories and every one was operated by persons of Swedish background. As their jobs turned out to be good ones, immigrants from Sweden encouraged their friends to come to America and to Rockford. Another factor in this concentration of Swedish people was the recruiting done by the Reverend Erland Carlson, Swedish Lutheran pastor in Chicago. Apparently he was a guiding light for immigrant Swedes coming to Chicago and he encouraged them to continue on west with the advice, "Don't get off the train until you reach the end of the railroad." The end of the road of course was the East Rockford station near Kishwaukee Street, and "Kishwaukee Street" came to be America's major locality in the minds of many people who lived in Sweden.[8]

[6] See Section A, part 1.

[7] It should be indicated clearly that in selecting this example the author does not contend that the Swedes are the only element worthy of note, for the Germans, Irish, Norwegians, and English also played important roles. But the Swedes came over in large groups and concentrated in Rockford. Consequently it is their contribution that can be traced most clearly.

[8] It frequently happened that letters from Sweden addressed to Swedish people "Kishwaukee Street, U. S. A." were delivered to the recipients in East Rockford.

Today over a quarter of Rockford's population are of Swedish extraction. Since almost all

The early success of Rockford's furniture industry established the city as one of nationwide industrial importance, a fame accountable primarily to Rockford's Swedes.[9]

Furthermore, many of Rockford's currently leading industries have stemmed from the furniture industry. The textile industry in Rockford was the result of inventions primarily by a Swedish woodworker (John Nelson). By 1900 the new industry was second only to furniture; 1377 of the city's 6620 manufactural employees were in furniture factories and 1246, in textile mills.

The hardware industry, now in first place in Rockford, was started by Swedes in the furniture industry. At first the furniture factories purchased their furniture hardware from New England. Near the turn of the century some of the Swedes in the furniture industry organized a company to produce furniture locks and hardware. Other items were added and other hardware companies were organized. Effective salesmanship and the production of satisfactory products has enabled Rockford's hardware industry to overflow its local market and compete successfully in the national market.

In starting Rockford's furniture industry, the Swedes provided an industrial impetus in the wake of which other industries have developed. The labor supply developed by the furniture, textile, and hardware industries served as a basis on which other industries could build. Since Swedes were instrumental in launching these three, it can be said that they played a major part in initiating the reservoir of labor which has expanded through the years.

Labor is not only productive, but also fairly stable. Labor turnover is small. Most companies do not have accurate records on percentage of turnover, or, if they do, they are reluctant to reveal the figures. By most of them, however, labor turnover was said to be "pretty small."

Men in industry in this region generally agree that there are numerous other localities in which their requirements for raw materials and markets would be satisfied less expensively than in the Rock Valley. They declare that labor is the Rock Valley's only attraction for their factories.

3. Wages Slightly Below Those Paid by Eastern Competitors

Some industries have been attracted to the Rock Valley by a wage level slightly lower than that in eastern industrial areas. This has been particularly true for the metal trades. Prior to the war, wage rates in most metal-working jobs were from

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of them live in East Rockford their proportion of population in that part of the city reaches 70%. Nearly three-quarters of the student body at East High School are Swedish. The 1948 city directory lists 3000 Johnsons.

[9] Swedes have played a prominent role in the American furniture industry. Three leading furniture producing cities, (Grand Rapids, Michigan; Rockford, Illinois; and Jamestown, New York) are areas of Swedish concentration.

The role of Swedes in America's furniture industry is discussed briefly by Helge Nelson (Professor of Geography, University of Lund) in his book, *The Swedes and the Swedish Settlements in North America*, p. 67.

5%–10% below those in Chicago; the differential was even greater for some cities farther east (Table III). Wage rates which are lower in the Rock Valley than other regions are not the result of suppression of wages. Rather, they reflect a lower level of living costs. It is a well known fact in economics that a differential in living costs exists between large and small communities. For one thing there is a lower tax rate in most small communities because of the lower cost of municipal government.[10]

TABLE III

RELATIVE LEVELS OF STRAIGHT-TIME HOURLY EARNINGS OF AVERAGE FACTORY WORKER OCTOBER, 1946
(median city equals 100)*

New York	120	Chicago	105
Detroit	115	Philadelphia	100
Pittsburgh	110	Milwaukee	100
Los Angeles	110	Baltimore	95
Cleveland	105	Boston	95
Buffalo	105	Rockford**	95

Source: "Trends in Wage Differentials, 1907–1947," *Monthly Labor Review*, Serial Number R. 1932, page 13 Bureau of Labor Statistics, U. S. Department of Labor.

* This table is based on the wage rates paid in 22 of the nation's largest cities. Wage rates in the median city are represented as 100; rates shown in the above table are percentages of the median city's average wage rate.

** Rockford was not among the cities listed in the Source. The entry for Rockford is based on field data procured by the author.

The fact that Rock Valley industrialists have had to pay for productive labor at a wage rate lower than that for eastern competitors has enabled manufacturers to pay freight on longer hauls for raw materials from eastern sources than eastern competitors must pay. Likewise on shipments of finished products to eastern markets, the Rock Valley fabricator can compete for the same reason. For example, a Cleveland machinery fabricator is nearer than a Rockford producer to both steel and a machinery market. Assuming that the Cleveland producer can procure steel locally at a base price of $100 per ton, and that the Rockford producer must pay freight on steel purchased in Chicago at the same base price, the comparative costs of the two producers competing in the Pittsburgh market for the sale of an 11,000-pound machine requiring 4000 hours to construct are as follows:

		Rockford producer		Cleveland producer
base cost of 5½ tons of steel		$550.00		$550.00
freight on 5½ tons of steel (carload rate)		25.30		
average hourly wage rate	(@1.44)		(@1.50)	
cost of labor (4000 hours)		5760.00		6000.00
freight to Pittsburgh		250.00		100.00
final cost delivered in Pittsburgh		$6585.30		$6650.00

[10] Harold M. Groves, *Financing Government*, First Edition, 1939, p. 513 ff.

An advantage in labor cost differentials enables Rock Valley manufacturers to overcome their handicap of location on the western frontier of the United States Manufacturing Belt.

4. Provision of Capital

Rock Valley people have supported industrial development by their willingness to supply capital. The role of capital has been three-fold: direct subsidy, provision of free land, construction of belt line railroads.

(a) Subsidies: The factor of financial loans and gifts has been cited earlier in the case of P. A. Peterson who gave away a fortune and still died a millionaire. Another case is in Beloit where townspeople raised $40,000 in 1929 to be used in developing Beloit's industry. Half of this was paid as a direct subsidy to persuade a company to move to Beloit from another city. It is doubtful whether the textile industry ever would have been started in Rockford if no one had financed the research of John Nelson and W. A. Burson; Ralph Emerson provided such support even before the two men began to produce on their knitting inventions. The willingness of Rockford banks to lend capital to industry is illustrated by the situation which prevailed prior to the depression of the 1930's when 60% of Rockford bank investments were in industry![11] When the depression struck, most of Rockford's eight banks were forced to close. If such a large part of their resources had not been provided to industry, fewer banks would have failed. Likewise, there would have been less industry in Rockford. Banks now follow a more conservative policy and would not approve over 10% of the loans which had been made to factories before the Depression. Today only 10%–20% of the banking assets are in manufacturing projects, but during an earlier period banks graciously provided capital, and factories blossomed in Rockford.[12]

(b) People in some Rock Valley communities have provided capital in the form of land. In Beloit a civic group spent $20,000 in 1929 to purchase land along Beloit's railroads to prevent that land from being occupied by low-grade residences, and to make it available as an inducement to factories. In Rockford a somewhat similar train of events occurred in the 1890's. A group of Rockford manufacturers headed by A. W. Brown induced the Illinois Central Railroad to run through Rockford. Brown then became the Illinois Central agent and played a prominent part in encouraging factories to congregate in southeast Rockford where he owned a farm, much of which was divided into plats and made available for factories.

(c) Hand in hand with provision of land in these two communities was the construction of railroad belt lines. Rockford and Beloit have the only belt lines in the Rock Valley, and many industries have been attracted by these facilities which were constructed as a result of efforts by the local citizenry.

The human factor has operated to a small degree through whim. Several fac-

[11] Mr. Eugene Abegg, President of Illinois National Bank, Rockford, in personal interview.

[12] For further discussion of the operation of this factor see W. D. Knight, *Subsidization of Industry in Forty Selected Cities in Wisconsin, 1930–1946.* Bulletin of the University of Wisconsin, Wisconsin Commerce Studies, Volume 1, number 2.

tories were located initially in the Rock Valley because the entrepreneurs thought the region was "a good place in which to live." This is one reason why the Parker Pen Company is in Janesville; George Parker was a resident of Janesville when he got his idea for fountain pens, he liked the city and its people, and decided to locate his factory there.

5. Summary of the Human Factor

The human factor in any region is difficult to appraise and measure in tangible form. The Rock Valley was settled by peoples who had achieved distinction in industry, yet similar nationalities settled most of the United States without going into manufacturing. The explanation here involves the early start which industry had in the Rock Valley. Some of the potentially industrial people did spread over the United States and enter non-industrial occupations, but in the areas where manufacturing was already underway they easily fitted into the industrial efforts. Had the early start of manufacturing in the Rock Valley been followed by an influx of people who were averse to factory work or who were not productive as factory labor, the region's industry would be much less significant. The major reason Rock Valley factories have made money is their people—an efficient leadership and productive labor—who compete successfully with companies better located in respect to raw materials, fuel, and markets.

C. LOCATION IN TERMS OF TRANSPORTATION COSTS

As far as manufacturers are concerned, location with respect to raw materials, fuel, and markets is not expressed in miles, but in either *time* or *cost*. For Rock Valley factories, proximity in terms of cost is the more critical.

There are three ways in which location of the Rock Valley as related to transportation costs has played a role in its industrial development: freight rate territories, basing points, and distance.

1. Freight Rate Territories

The Rock Valley is located barely within the western margin of the lowest freight rate territory in the country. The five freight-rate territories in the United States are shown in Figure 3.[13] Freight rates are cheaper mile for mile in the Eastern or Official territory than anywhere else in the United States; however, the advantage of a location within Official Territory is decreasing. The current trend is gradually to erase the freight-rate differences. If recent actions of the Interstate Commerce Commission are an omen, the time will come when the freight-rate advantage of the Eastern Territory will be reduced to a small margin, if any.

[13] For a brief presentation of this freight rate structure see U. S. Government, 75th Congress, 1st Session, "*The Interterritorial Freight Rate Problem of the United States,* House Document #264.

U. S. Government, 76th Congress, 1st Session, *Supplemental Phases of the Interritorial Freight Rate Problem of the United States,* House Document #271.

FIG. 3.

Figure 3 is a map of "class rates." Most freight moves under "commodity rates"; however, these are so complex that it is impossible to construct a map of them. In general, however, the level of "commodity rates" approximates that of "class rates" in each of the regions shown in Figure 4.

2. Basing Points

The Rock Valley's locational disadvantage on the western fringe of the United States Manufacturing Belt was diminished by the multiple basing point system of determining steel prices. In this system, the cost of steel to a consumer was based on the price of steel at the nearest basing point plus freight from mills at that particular basing point to the consumer *regardless of where the steel actually was produced and shipped.* South Chicago was the basing point nearest Rockford, Beloit, and Janesville; consequently fabricators in these three cities could purchase steel from any producer in the United States at the identical price they would pay in south Chicago.[14] Their location 120 miles from south Chicago was as good as a location 120 miles from Pittsburgh, the nation's leading area of steel production. This system nullified the advantage which many eastern fabricators had over western factories for proximity to steel.

In July 1948 the United States Supreme Court ruled that a similar basing point system operated by the cement industry was illegal. The steel industry caught the cue and, without waiting for a court struggle, quietly abandoned the multiple basing

[14] The steel industry maintained practically the same base price at all basing points.

point system in favor of a f.o.b.[15] system. It still is too early to determine the effect of this new system on Rock Valley metal industries, but the consensus of opinion among the region's industrialists is pretty well summed up in the words of Mr. C. M. Oberling (Purchasing Agent of the National Lock Company, Rockford) who said, "It is certain that we will not be helped. It is too early to tell if we will be hurt."

3. *Distance*

Suppose the Rock Valley with all its industrially inclined people, with its long record of manufacturing, and with its same railroad pattern and connections were suddenly transposed to western Wyoming. The industrial complexion of the Valley would fade rapidly, because basic to the Valley's industrial growth is its location *near enough* to markets and raw materials so that transportation costs are *not prohibitive.* This is not to minimize the locational disadvantage which the Rock Valley has in terms of distance from coal, pig iron, steel, leather, cotton, wool, wood, other raw materials, and the major United States market. Eastern industrial regions are nearer than the Rock Valley to all of these; however, the Rock Valley's position still is not so remote that freight costs prevent it from competing. The advantages of productive skilled labor at lower costs are not outweighed by the disadvantage of transportation costs on shipments over great distances.

This factor of transportation costs is not distinctive for the Rock Valley in comparison with the rest of Illinois and eastern Wisconsin. However, it does help explain why industries which are not oriented locally can exist at all.

D. ADVANTAGES INHERENT IN SMALL CITIES

A small city generally offers to industry four particular advantages: lower tax rates, lower rents, more pleasant living conditions, and less disturbed industrial relations.

Tax rates in small cities usually are lower than those in large cities because the cost of government is less.[16] Rockford, Beloit, and Janesville have much lower tax rates than Chicago and Milwaukee.

Rents often are lower in small cities. For example, a company in Chicago was paying 80 cents per square foot for 8000 square feet of space in upper stories of a building in a crowded neighborhood. In 1947 the company moved to Rockford where it found identical space on the second floor of a building located on the fringe of the central business district one block from a residential area and renting for 40 cents per square foot per year.

Residents of the Rock Valley cities are outspoken in stating their preference for small communities over big cities for residential purposes. Many of the factory employees go home for lunch. Most of their homes have lawns and gardens. As one man said, "This is a swell place in which to live and bring up a family. You can't get *me* to go back to Chicago."

[15] Free-on-board at loading point with the customer paying freight costs.
[16] Harold M. Groves, *op. cit.*

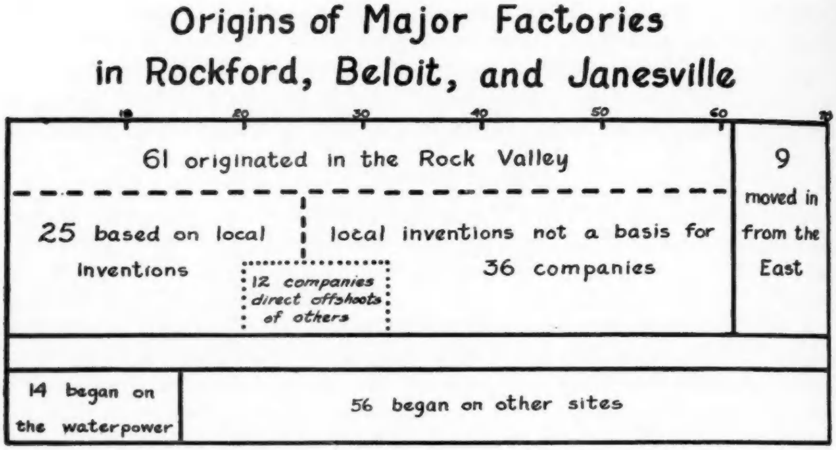

FIG. 4. United States Freight Rate Territories.

Index of Relative Levels:

Eastern or Official	100%
Southern	139%
Western	147%
Southwestern	175%
Mountain–Pacific	171%

Source: United States Government, 75th Congress, First Session, *The Interterritorial Freight Rate Problem of the United States* House Document 264 Chart 4.

Industrial relations are less disturbed in small communities. Prior to 1937 (the Wagner Act), strikes were almost unknown in the Rock Valley. In the last forty-three years Beloit has had only one strike, Janesville has had two, and Rockford has had seven.

E. MISCELLANEOUS FACTORS

There are several other factors which permit manufacturing in the Rock Valley but do not explain why it is localized there. The climate is favorable. Local relief is low. Railway transportation is adequate. The Rock Valley cuts across the grain of railway routes radiating from Chicago and Milwaukee. There are ten major routes extending west from Chicago and crossing the Rock Valley; but there is no

[17] The importance of railroads to a community's industrialization is illustrated by the history of Grand Detour, located on the Rock River forty miles downstream from Rockford. "A grist mill was completed at Grand Detour in 1839, and from 1840 to 1855 it was the largest trade and industrial center in the county and one of the most important in the Valley. The great mistake made by the citizens of Grand Detour was to oppose the coming of the railroad under the mistaken idea that its business and manufacturers would thereby be dissipated among various upstart towns. So the railroads passed her by, and left her almost a deserted village. . . . Even the local post office was eventually discontinued and Grand Detour received its mail by rural delivery from Oregon."—R. B. Way, *The Rock River Valley*, p. 571.

line running through the Valley for more than a small portion of its length. If the railroads had not come to the Rock Valley, the region never could have been industrialized to the degree it now is.[17] However, it cannot be said that "good railroad service" is an advantage distinctive to the Rock Valley. Other midwestern cities such as Champaign, Bloomington, Mendota, LaSalle, and Kankakee, have just as good rail service as the Rock Valley, yet they are not such intense manufacturing centers.

CONCLUSION

In conclusion, some of the ways in which present-day manufacturing companies originated in the Rock Valley can be summarized. In Rockford, Beloit, and Janesville there are a total of seventy factories employing 100 or more people, accounting for 85% of the cities' industrial employment. Sixty-one of these companies were born in the Rock Valley while only nine moved in from elsewhere (the East). A few of the factories of local origin have been subsequently purchased by outside interests. Twenty-five of the sixty-one native factories can be traced directly to local inventions. The other thirty-six employed familiar manufacturing processes. Twelve of the seventy major factories are direct offshoots of local companies. Fourteen of the seventy were started "on the waterpower" of their respective communities (Figure 4).

NOTES ON THE DISCOVERY OF YEZO

JOHN A. HARRISON
University of Florida

UNTIL the middle of the nineteenth century, the state of Western cartography concerning the seas and islands north of the Japanese island of Honshu was one of almost complete ignorance. Lindner has aptly described that area as ". . . ein geographischer Rätsel . . ."[1] For almost as long a time, the region was just as much a riddle to the Japanese themselves, despite centuries of contact and colonization. However, this native deficiency cannot be accounted for by that ignorance of the Japanese language which led the Western world astray, for, while everyone used the word *Yezo* in reference to this area, no one knew exactly what *Yezo* meant or designated. Many European cartographers had their own ideas on this subject which they proceeded to translate into the peculiar and amorphous shapes, which, in the maps and charts of the sixteenth to eighteenth centuries, fill the space north of Honshu and east of the Okhotsk coast. It was generally believed that *Yezo* designated a large island mass, but there were those who held it to be a series of islands. This conflict of ideas was well described by a contemporary Dutchman who wrote that "*Jezo* is generally esteemed to be an island . . . though there are some who call that into question . . ."[2] As a matter of fact, what the Japanese meant by *Yezo* was the entire area north of their advancing frontier.

Before the Nara period (710–782) all of that area which lay to the eastward of, roughly, the 135th meridian was called *Koshi* by the Japanese as were the non-Japanese peoples living in that area.[3] As time passed and many of these people were conquered or assimilated that name became restricted to Northern Honshu and the areas beyond, and centuries later that name still lingered in the Kuriles where the natives were known as *Kushi,* a corruption of *Koshi.*[4] But for general use an important linguistic change took place which converted the word *Koshi* to *Kui* or *Kai.*

[1] "Seidem die Europaer die erste Nachrichte von dem Lande Jesso erhlielten is desselbe ein geographischer Ratsel gewesen." Ludwig Lindner, "Entdeckungsgeschichte der Insel Jesso und der Halb-insel Saghalien," *Allegemeine Geographisches Ephmeriden,* XXXVIII (1812), 249.

[2] Ashwan and John Churchill ed., *A Collection of Voyages and Travels* (London, 1704), IV, 534–535.

[3] Much of the following material on the derivation of the word *Yezo* is taken from the original and unpublished work by Mr. Ensho Ashikaga, of the University of California at Los Angeles, on the Races of Ancient Japan, who has generously permitted me to use it.

There is no real agreement among Japanese authorities on this word. Professor Kita Sadakichi of Tokyo Imperial University says that the Ainu used to call themselves *Kai* and that there were numerous Japanese words for them such as *Kai, Kushi, Kui, Kaina and Ainu* and that originally the Emishi or Ebisu designated a wide range of non-Japanese peoples in Old Japan. On the other hand Professor Kindaichi Kyōsuke says that the Ainu originally termed themselves as *Enju* and that the Japanese adopted this word as *Emishi.*

[4] S. Krashnennikov, *History of Kamchatka and the Kuriles,* trans. James Grieve (Gloucester, 1764), 33–34.

When that name came to be written down, the compound of characters by which the Chinese referred to the *Kai* as an "eastern barbarian people," was adopted by the Japanese with the pronounciation *Ezo* or *Yezo*.[5]

Yezo, in its English, Spanish, Portugese and Dutch variations, is the term with which early Western navigators of the Northern Pacific were familiar yet they did not know that it designated the island of *Hokkaido* (*Higashi* or Eastern *Yezo*), Saghalien (*Kita* or Northern *Yezo,* or *Oku* or Upper *Yezo,* also better known as *Karafuto*), the Kuriles (Upper *Yezo,* also better known as *Chishima*) and even, at times, the peninsula of Kamchatka. Indeed, despite the plenitude of local names for these regions, the term *Yezo* was used to denominate the entire northern seas complex.[6] Unfamiliarity with this usage could lead as able a navigator as Krusenstern to search for an island named *Karafuto* refuted to lie between *Yezo* and Saghalien while the famous John Bell concluded that Kamchatka ran from the Amur to the northeast point of the continent of Asia.[7]

But while the West never saw a really accurate map of *Yezo* until about 1852, the Japanese had been familiar with the area since before they recorded their own history. Prior to the Nara period, colonization of the *Yezo* border had begun to take place as a series of scattered border stations or outposts against Ainu raids.[8] The *Nihongi* or Chronicles of Japan (720?) contains many references to Ainu-Japanese conflicts while later, in the eighth and ninth centuries, there was almost continual warfare along the border, with the Ainu, at times, driving as far south as the *Kantō* or great plain north and east of modern Tokyo.[9]

Northern Honshu came under Japaense control by the middle of the eleventh century and was dominated by the Abe family which turned the domain into a semi-independent power far greater and more extensive than any other in Japan. But the Abe clan retained only a vague connection with the unknown island to the north,

[5] The characters can also be read as *Ebisu,* a corruption of the word *Emishi.* The latter word is derived from either the *Kai* word "yumasu" meaning "sword" or the Ainu word "emush" also meaning "sword" and was presumably used to refer to the military qualities of these peoples. The derivation of *Yezo* is still largely an unsolved problem. The material in this article is probably as accurate an approximation as any. At present Prof. Takakura Shinichirō of Hokkaido Imperial University at Sapporo is working on a new and more definitive solution of this linguistic problem.

[6] In this paper the word *Yezo* will be used to denote both Hokkaido and the entire maritime area bounded by Honshu, Kamchatka and the coast of the Okhotsk sea to the Amur mouth.

[7] John Bell, *Travels from St. Petersburg in Russia to Diverse Parts of Asia* (Glasgow, 1763), I: 241.

[8] Kamikawa Taisuke, *Heian Chōshi* (A History of the Heian Court), (Tokyo, 1930), 41ff. If Hayashi Shihei, the Tokugawa historian, is to be believed, the Japanese had brought the very southern tip of Yezo under control in 658.

[9] These incidents are recorded in: W. G. Aston trans., *Nihongi, Chronicles of Japan from the Earliest Times to A. D. 697, The Transactions and Proceedings of the Japan Society of London,* suppl. I (London, 1896); Robert Karl Reischauer, *Early Japanese History (ca. 40 B. C.–A. D. 1167)* (Princeton, 1937), Part A. The most dependable Japanese monograph is Maruyama Jirō, *Yemeshi Hayato no Jumbu* (The Pacification of the Yemeshi and the Hayato), (Tokyo, 1934).

although there had been trade with Hokkaido ever since the seventh century in fish, fur, and feathers. In the twelfth century scattered Japanese settlements appeared in the *Ōshima* peninsula of southwest Hokkaido as refugees from the wars of the *Minamoto* fled northward. This peninsula became a fief of the central government in the fourteenth century when the *Kamakura* Shogunate, anxious to exercise some kind of control over the far frontier gave it to the Ando family. But the Ando were not able to hold Ōshima due to Ainu revolts, and the job of reconquest was undertaken by a samurai named *Takeda Nobuhiro*. He re-established the Japanese grip on that part of the island, assumed the lordship as his reward, and changed his name to *Kakizaki*. This occurred sometime around the year 1442. Since the distance from the capital of Japan to *Yezo* was great and since Japan was being torn by civil war, it was easy for the *Kakizaki* to reign in *Yezo* as practically independent princes.[10]

However, in the sixteenth century, the *Kakizaki* bowed to the inevitable and received confirmation of their fief from Toyotomi Hideyoshi, the great military conqueror and unifier of Japan. At this time they changed their name to *Matsumae* under which name they held *Yezo* in fief during almost all of the period of the Tokugawa Shogunate (1615–1868) and by which name the island of Hokkaido was known to the West.[11] The conditions of government under the *Matsumae* were such that any mapping of Hokkaido or penetration of the areas still further north and west was an impossibility. Japanese settlement in the island was restricted to the tip of the Ōshima peninsula. The object of this restriction was to placate the restless Ainu interior by keeping Japanese away from aboriginal territory. This demarcation was also prompted by an economic motive. The prosperity of the area and hence of the Matsumae lay in its fisheries and in the fur and hide trade. The frontier was open only to fur traders and fisheries contractors. The income of the Matsumae was based largely on contractors fees. Since these contracts were let on an auction basis, the contractor who was highest bidder had to squeeze the abnormal profits he expected out of his Ainu fishers and hunters, and since there was no police nor military system, the contractor's treatment of the natives was notoriously bad.[12] The natural result of such a system was a series of native revolts which were suppressed only with great difficulty and which made the interior of Hokkaido even more *tabu* than before. As long as the Matsumae continued their negative policy so long was *Yezo* a *terra incognita* to the Japanese. Thus the impetus given to Japanese mapping of Hokkaido, Saghalien, and the Kuriles was to come from the outside—from the West.

In the sixteenth century Europe had heard something of the maritime northeast

[10] Takaoka Kumao, "Die Inner Kolonisation Japans," *Staats- und Sozial-wissenschaftliche Forschungen*, XXIII (1904), 5ff.

[11] *Matsumai* is probably a corruption of the old Ainu term *Madomai* which designated that part of the island of Hokkaido south of the Ishikari Valley. J. Batchelor, "Notes on the Ainu," *The Transactions of the Asiatic Society of Japan*, X (1882), 218–219.

[12] Minami Tetsuzo, "Matsumae Han Jidai ni okeru Bashō Ukeoi Seido" (The Contracting system in the Matsumae period) *Shakai Keizai Shigaku*, III: Part 5 (August, 1933), 465–476.

of Asia and it was extremely interested in what it heard—that this area abounded in gold and silver. The voyages in search of the Gold and Silver islands were to place Yezo on European maps and on Japanese maps.[13] The Western voyages of discovery in this area may be likened to two wings converging, one from the North and one from the South. The southern or Dutch-Spanish-English wing has been fairly well treated in historical and geographical literature. The northern or Russian wing which was equally important has been sadly neglected by both historians and geographers.

Western Europeans evolved the myth of the fabulous Gold and Silver Islands, but it seems to have penetrated even to Japan for the English pilot Will Adams was requested by the Shogun Tokugawa Iyeyasu to undertake a voyage northward in order to discover lands rich in gold and silver.[14] The earliest mention of *Yezo* itself, seems to be in 1556 when an unnamed Jesuit is reported as saying that *Yeso* was a body of land north of Japan,[15] but the great Jesuit missionary Froez was probably the first Westener to give any description. In one of his letters, written sometime between 1570 and 1580, he notes that there is an island of hairy people some three hundred leagues to the north of Japan although he does not mention the existence of any precious metals, merely stating that these people lived by barter.[16] Nor did the Friar Hieronomius de Angelis, first European to visit *Yezo* itself (1620? 1622?) speak of riches there.[17] It was another priest, Andres de Aguirre, who, in a letter to the Governor of New Spain, mentioned that there were islands of gold and silver somewhere in the Northwest Pacific. In 1609 the historian Antonio de Moraga in-

[13] The only Western work devoted to a study of these early Spanish and Dutch voyages for the Gold and Silver Islands is: Oscar Nachod, *Ein Unentdecktes Goldland; Ein Beitrag Zur Geschichte Der Entdeckung Im Nördlichen Grossen Ocean. Separatabdruck aus den Mitteilungen Deutsche Gesellschaft für Natur- und Völkerkunde Ostasiens* (1900).

[14] James Murdoch, *A History of Japan from the Origins to the Arrival of the Portuguese in 1542 A. D.* 2nd ed. (London, 1925), 616.

[15] Recueil de Voyages au Nord, (Amsterdam, 1732), IV: 20.

[16] Richard Hakluyt, *The Principle Navigations, Voyages, Traffiques & Discoveries of the English Nation, made by sea or overland to the remotest and fartherest distant quarters of the earth at any time within the compass of these 1600 years.* (Glasgow, 1904) VI: 33.

P. F. von Siebold in his wonderfully accurate *Geographical and Ethnographical Elucidations to the Discoveries of Maerten Gerrits Vries Commander of the Flute Castricum A. D. 1643: To serve as a Mariners Guide in the Navigation of the East Coast of Japan and to Jezo, Krafto and the Kuriles.* Translated from the Dutch by F. M. Cowan (London, 1859), places the earliest mention at 1565—see footnote on p. 36—but does not mention the writer of the letter. Further on in this work von Siebold notes that the first Western book devoted to Yezo was the *Korte Beshrijvinghe van het Eylandt Ezo enz* (Amsterdam, 1646).

[17] Pere de Charlevoix, *Histoire et Description generale du Japon* . . . (Paris, 1736), I: 248. Reported also by Nicholas Witsen as being 1622. See Part II, p. 57.

Leon Pagès in his *Bibliographie Japonaise depuis le XVe siecle jusqu'a 1859* cites a letter *sur Yesso* written in 1620 by P. Diogo de Cawalho. Fr. von Wenckstern in his *Bibliography of the Japanese Empire* cites the same letter as being written *in Yesso.* The difference is slight but unless one reads this letter of which the only copy is in the Vatican archives one cannot be sure whether Father Diogo was writing from Yezo or concerning Yezo and Charlevoix must be accepted as the authority for de Angelis' being the initial European there.

cluded this information in his general description of Eastern Asia.[18] A few years later, in 1613, John Saris, head of the English factory at Hirado, wrote that ". . . this island of *Jedso* hath Gold, Silver and other riches."[19] This seems to be the first equation of *Yezo* with the Gold and Silver islands although in 1598 Ortelli shows an "Isla la Plata" between 40° and 50° North and 160° and 170° East in his *Theatrum Orbis Terrariem* No. 6. It was an age of discovery when no reputed treasure seemed improbable and the active Spanish commercial empire sent Pedro de Unamuno to search for these islands of Gold and Silver. He found nothing and concluded that the report was in error and that such islands did not exist, but in 1611 Spain sent Sebastiano Vizcaino to search for them, combining this effort with his mission as envoy to Japan. Vizcaino failed in his goal and the Spanish search for *Yezo* ended, but the European sweep of the northwest Pacific was continued by the Dutch. Verstegen, who read Moraga's work, sent a report (1636?) to the directors of the Dutch East India Company to the effect that there were rich islands in the Pacific in the approximate lattitude of 36½° North.[20]

This report of Verstegen's resulted directly in the famous voyages of Tasman and Quast in 1639 and that of Vries in 1643 (Fig. 1). None of these captains found the Gold and Silver islands, but Vries discovered *Yezo*. He mapped part of its eastern and northern coast and took possession of the Kurile Island of Uruppu in the name of the Dutch East India Company. He also was to add to the legends of wealth when he wrote in his log that a native of the Ushibetsu river country had told him that the mountains of the interior were full of silver.[21] Unfortunately for the world of geography this first accurate report on *Yezo,* the log of Vries, was lost in the archives of the DEI until 1858.[22] Partly due to this, there persisted that confusion in European cartography which has already been mentioned, no one being sure whether *Yezo* was a term for Northern Japan, Tartary or America, or, indeed, whether *Yezo* was a part of Japan. While the log of Vries would have done much to clarify this confusion there did exist a contemporary work which, if more carefully studied, would have helped resolve the issue. Kaempfer had written in 1692 that the Japanese referred to the entire North as *Yedso,* while to the island of *Yezo* proper, they gave the name *Yessogasima.*[23]

[18] H. E. J. Stanley trans., *The Philippine Islands, Molluccas, Siam, Cambodia, Japan and China at the Close of the 16th Century* (London, 1868), 41, 229, 397.

[19] Samuel Purchas, *Hakluyt Posthumous or Purchas His Pilgrims Containing A History of the World in Sea Voyages and Land Travels by Englishmen and others* (New York, 1905-1907), I: Book 4: 384. In line with this another merchant, Caron, senior Dutch factor in Japan, reported in 1636 that Japan was rich in gold, silver, and copper, and that *Jesso,* twenty-seven days to the north of Edo, abounded in furs. John Pinkerton, *General Collection of Voyages and Travels* (London, 1811), VII: 609, 639.

[20] J. E. Heeres trans. and ed., *Abel Janzoon Tasman's Journal* (Amsterdam, 1898), Part 3: 15.

[21] Siebold, *op. cit.,* 35.

[22] Charlevoix, the early Jesuit historian of Japan, thought so little of the discovery as to say that the Vries voyage was of little consequence! The log was read by the DEI which also thought the discovery of no moment.

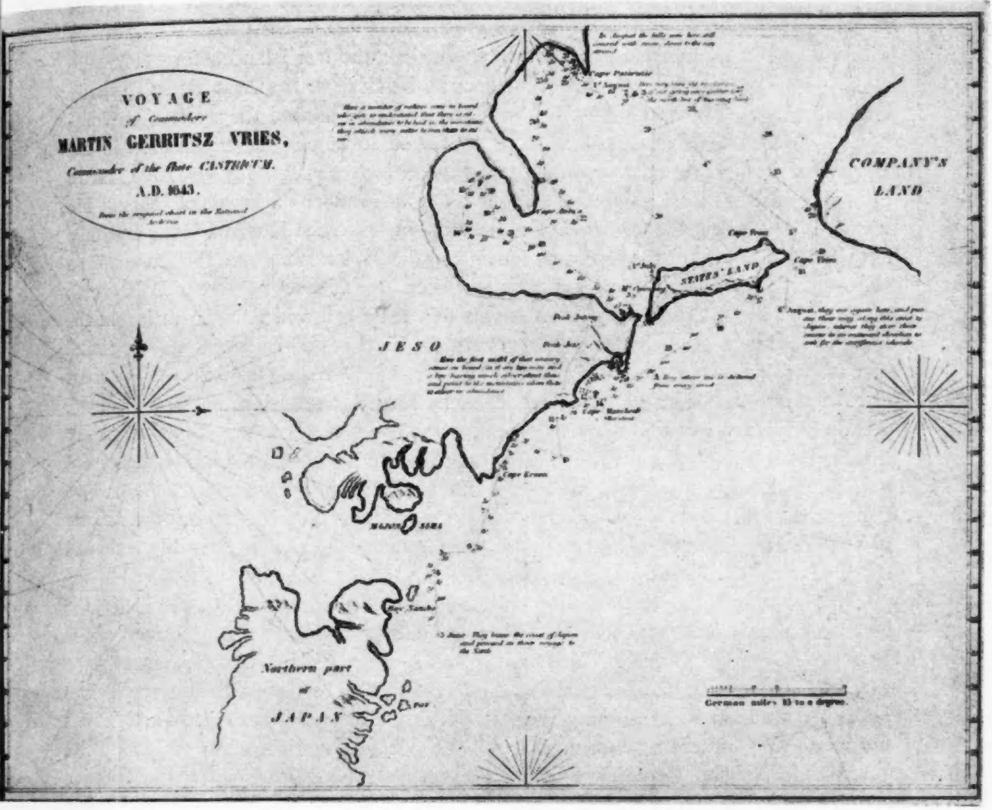

FIG. 1. A reproduction of Vries' map (1643).

The next great voyages from the south were not to come for over a century when both LaPerouse and Broughton were to attempt to chart that area but meanwhile the Russian or northern wing of the search was to contribute to the world more accurate knowledge of Hokkaido, Saghalien and the Kuriles.

The Kurile islands provide an easy line of communication between Japan and Kamchatka with no island at a distance greater than one hundred miles from its neighbor. The early establishment of Russian Cossack posts in Kamchatka (1696) was soon followed by exploratory trips, in small boats, to the islands lying south of Kamchatka, and by 1711 the islands of Shimushiro and Paramushiro had been ex-

[23] Engelbert Kaempfer, *The History of Japan together with a Description of the Kingdom of Siam 1690–1692.* Trans. by J. G. Scheuchzer (New York, 1906). The translator's introduction compounds a common error by referring to Kamchatka as *Yezo (Okuyesso).*

plored.[24] Since Kamchatka provided neither raw materials nor foodstuffs, the Russians had to push out both east and south, and in 1712 the Cossack Kosirewski went down the Kurile chain to Kunashiri which is the last island before Hokkaido itself. The Cossack reports that went back to Russia got the Tsar Peter I greatly interested in the ore possibilities of the Kuriles and he pushed the work of exploration.[25] After his death, his successors continued to emphasize exploration in the *Yezo* area with trade with Japan as the prime objective. In May of 1739, Martin Spangberg set sail from Kamchatka under orders to explore the coasts of Japan. He was precisely ordered to "examine and describe in detail the Kurile Islands and then to proceed to Japan for the purpose of arousing their friendship with the Russians, so as to destroy their deep-rooted Asiatic seclusion."[26] He set sail from Bolsheretsk in a southeasterly direction because it was generally believed that Japan lay in the same meridian as Kamchatka, but, reaching the 73rd parallel without sight of land, he changed his course to southwest and on June 18, 1739 he anchored off the coast of Mutsu province, Northern Japan. He was cordially received and reprovisioned, and on his return voyage he coasted *Yezo*. Spangberg returned to Russia confident that he had found Japan and the water route thereto but the Russian Admiralty did not consider his proof convincing. His charts placed Japan some eleven to twelve degrees west of where it was on the Admiralty charts. For this reason the reports of Spangberg's voyage were not believed to be accurate and it was generally reckoned that he had found Korea and not Japan.[27]

It is not easy to understand this attitude of the Russian Admiralty. Mueller, the historian of Siberia, states that Spangberg's findings conflicted with the map of the Russian empire then in use. This map, (Fig. 2) prepared by Kirilov, was, to some extent, based on the *Description de l'Empire Russien* of Count Strahlenburg first published in 1730.[28] Assuming that the Kirilov map was inaccurate—and it was not a bad map—there was no need in 1739 to depend upon it. As early as 1714, Joseph Delisle had drawn a fair map of the Northern Pacific for the Russian Academy of Science. In 1731, this map was revised for the use of Vitus Bering.

[24] Strangely enough the Cossack commanders, Lucas Morosco and Vladimir Atlasov received from the Koriaks of Kamchatka, in 1697, a piece of gold which the natives claimed came from Japan and which helped give the Cossacks a grandiose impression of the lands that lay to the south of Kamchatka. This impression was reinforced when a Japanese castaway found on Kamchatka related that in his home land (he was from Osaka) the roofs of the temples were sheathed in gold. Tabohashi Kiyoshi, *Kindai Nihon Gaikoku Kankei Shi* (A History of the Foreign Relations of Modern Japan) (Tokyo, 1930), 67–77. Horie Takeo, "Genroku Kōhō Nenkan Roshiya ni okeru Nihonjin" (Japanese in Russia in the period 1694–1735), *Shigaku Zasshi*, XXIX (1918), 1111–1130.

[25] G. Mueller, *Voyages from Asia to America, for completing the Discoveries of the North-West Coast of America. To which is prefixed A Summary of the Voyages made by the Russians on the Frozen Sea, in search of a NorthEast Passage*, trans. by T. Jeffreys (London, 1761), pp. xxxviii, xliii.

[26] S. Novakovskii, *Iaponiia i Rossiia* (Tokyo, 1918), 28–29.

[27] Mueller, *op cit.*, pp. 33–34.

[28] Translated from the Russian and published at Amsterdam in 1752.

FIG. 2. Kirilov's map used by Russia (1730).

Although Mueller claims that this second map was not done until mid-1732, it would still mean that seven years prior to the voyage of Spangberg, the Admiralty had evidence of the correct relationship of Kamchatka, Korea, and Japan, which is clearly shown on this map, and with this evidence before them, it is difficult to understand their complete rejection of Spangberg's work. However, Delisle's outline of Yezo is not correct. He had used Vries' report and the 1687 map of Witsen, and, although he shows a large island east of Saghalien named "Terre d'Eso," he had given a continuous coastline to Hokkaido and the Kuriles. Mueller attacks Deslisle for refusing to utilize the early Russian reports of a group of islands lying between *Yezo* and Kamchatka.[29]

The real impact of Spangberg's work is to be found in Edo not St. Petersburg.

[29] Mueller, *op. cit.*, pp. 39, 20. The work of Deslile has been collected in the *Atlas Russien: contenant une Carte Générale et 19 Cartes Particulieres de tout l'Empire de Russie et des Pays Limitrophes constituées, conformément aux regles de la Geographie et aux dernieres Observations par l'Academie Impériale des Sciences* (St. Petersburg, 1745). A complete listing of Delisle's maps will be found in J. Isenard, "Joseph Nicholas Delisle sa biographie et la collection de cartes geographiques a la Bibliotheque Nationale," *Comite du Travaux historique et scienti-*

On June 8, 1738, orders were issued by the Shogun calling on the coastal barons for a stricter sea defense. This move came some ten days before the appearance of Spangberg off the coast and was prompted by a steadily increasing flow of reports from the North that foreigners were trading in the Kuriles. There was apprehension in Japan at the idea of a weak and undefended frontier lying close to a powerful neighbor and this apprehension was not diminished by the anti-Russian calumnies of the Dutch factors at Nagasaki. Despite the lack of detailed geographic information, Japan now had intimate contact with Hokkaido, Saghalien, and the Kuriles, and the value of this region was highly esteemed by the Shogunate. The Matsumae had held dominion over *Yezo* for two centuries. Since 1672, they had maintained fishing stations on Saghalien, and by 1700 there were twenty-two Japanese settlements on that island.[30] Several times surveying expeditions had tried to map both *Yezo* and *Karafuto* but had failed due to winter weather and the great chain of scarps that crosses both these islands.[31] By the middle of the eighteenth century, the *Yezo* area was so well considered that the prospect of losing the loosely held northern marches gave rise, among Japanese statesmen and scholars, to a debate on the best methods for defending this "Key to the Northern Gate" (Hokumon Sayaku).[32] News of the penetration of the Kuriles and of occasional Russian landings in *Yezo,* as reported by the Matsumae to Edo increased the nationalistic fervor that had already been engendered by the scholar-noble Tokugawa Mitsukuni with his new and highly nationalistic history of Japan and by the scholar Motoori Norinaga with his chauvinistic attack on Chinese learning and culture in Japan. This is no place to discuss the new school of nationalistic literature in Japan occasioned, in part, by the proximity of a new and unknown power to an unsettled frontier but suffice it to say that the Shogunate aroused by both the internal danger—for these works were essentially an attack on the Tokugawa system—and the foreign danger commenced an examination of that literature extant in Japan which was descriptive of *Yezo.*

Despite centuries of trade and contact, there was little to go upon. Not until near the end of the eighteenth century did there arise that body of Japanese materials relative to *Yezo* which has been so admirably catalogued by Chamberlain.[33] No real maps existed. What evidential material the Shogunate had on hand was inadequate to fill the lacunae. Two prominent works on *Yezo,* the *Ezo-dan Hikki* (Notes on

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fique Bulletin de la Section Geographique, XXX (Paris, 1916), 34–164. Delisle's defense of his work will be found in H. M. Omont, "Lettres de J-N Deslisle au Comte de Maurepas et a l'abbe Bignon sur ses travaux geographiques en Russie (1726–1730)," *Bulletin de la Section Geographique,* XXXII (Paris, 1918), 130–164.

[30] Y. Takekoshi, *The Economic Aspects of the History of the Civilization of Japan* (London, 1930), III: 181.

[31] [Hayashi Shihei]. *San Kokf Tsou Ran To Sets,* trans. J. Klaproth (Paris, 1832), 192–193.

[32] Tokutomi Iichirō, *Bakufu Bunkai Sekkin Jidai* (The period approaching the final decline of the Bakufu) (Tokyo, 1927), 323.

[33] Basil Hall Chamberlain, *The Language, Mythology and Geographical Nomenclature of Japan Viewed in the Light of Ainu Studies. Memoirs of the Literature College of Imperial University Japan* (Tokyo, 1877), 137–174.

Yezo, 1704? 1711?) of Arai Hakuseki and the *San Koku Tsuran* (Retrospect of Three Countries, 1785) of Hayashi Shihei, dwelt more on the Ainu and the legendary wealth of *Yezo* than on descriptive geography. From the *Ezo-dan Hikki*, we draw one valuable nugget of information. The distance from the island of *Yezo* to the seal rookeries in the Kuriles was not known, nor, to the author's knowledge, had any Japanese been there. This, despite the extensive Ainu trade between that chain and Yezo, must have been true, for succeeding evidence points to the fact that not until Japanese coasters were driven ashore by storms did the Japanese get to the Kuriles.[34] The other work mentioned, the *San Koku Tsuran,* starts with those mentions of *Yezo* that occur in the *Shan Hai Ching,* a collection of pre-Han fragments. The author then draws a picture of *Yezo* and its relationships to Japan in his times. According to Hayashi there lay to the northeast of Tartary an island beyond which were the Islands of the Night. To the north of *Yezo* itself lay Karafuto. Across from Karafuto was Manchuria. Between Yezo and Kamchatka were thirty-seven Kurile islands. The author produces a map (Fig. 3) to illustrate his point, perusal of which leaves his accuracy open to question. Besides thirty-seven Kurile islands, he has Kamchatka as a peninsula jutting far south of the Amur mouth and a distorted island of Yezo. Now Hayashi mentions the *Matsumae Shi* (History of the Matsumae) written by Matsumae Hironaga. This work on the geography and history of Yezo had a map prepared by a clan retainer named Kato who had travelled in Yezo and Karafuto in 1751 and so was acquainted with the area at first hand. His map is accurate, indeed so amazingly good that later students have concluded that he received a great deal of information from the Dutch at Nagaski and the Russians in Yezo. How otherwise explain that he was able to show Alaska in correct outline and relationship to Japan as well as the voyage of Bering. The map was published in 1781. It is hard to understand Hayashi's complete deviation from its information in his own work of 1785.

The *San Koku Tsuran* divides the island of Yezo into five provinces with a total of 123 hamlets which did a brisk business in game, fur and feathers. The mountains of Yezo, the author asserts, contain gold as do the sands of the shores and the beds of the rivers.[35]

Part of this material had come from the accounts of early expeditions sent to map the islands. In the time of Hideyoshi (1586–1598), one such party had spent two years unsuccessfully trying to cross Yezo from south to north. In the time of the Tokugawa Shogun Hidetada (1605–1622), a party had tried to map Karafuto but had turned back due to the severe winter weather. In 1719, the Shogun Yoshimune had ordered a new survey of Japan made, but no one was able to include a map of Yezo.[36] At the end of the eighteenth century, Tokugawa Mitsukuni had undertaken

[34] The effects of the seclusion laws on Japanese sea voyages should be noted. Great restrictions were placed on tonnage, construction and routing so as to ensure only coastwise trade. This was detrimental to the development of navigation and of maritime discovery by the Japanese.

[35] Klaproth, *op. cit., et passim.*

[36] Ōtani Ryōkichi, *Tadataka Inō The Japanese Land Surveyor,* trans. Kazue Sugimura (Tokyo, 1932), 25.

to have a survey of Yezo made at his expense, but, as before, such an effort proved abortive. When the Shogunate assumed direct control of the island of Yezo in 1798, there was not a single existing chart of the coast of Yezo, nor, for that matter, of Honshu from Edo north to Aomori.[37] Not until 1800 was an accurate map to be started and then, as if to make up for the failure of preceding generations, two of Japan's greatest geographers started to explore Yezo.[38]

A small merchant, Tadataka Ino, who by hard work amassed enough money to indulge his bent for the new Dutch scientific learning, was the first man to really map Yezo. Late in life he retired from business to devote the remainder of his existence to his hobbies of astronomy and mensuration. From 1800 to 1818, at his own expense and with occasional help from officials, he explored and charted Yezo and Japan.[39]

His labors in Yezo were carried on by his pupil Mamiya Rinsō, a young official in the Tokugawa bureaucracy in Yezo. Rinsō's duties as inspector in the Department of Yezo Affairs included occasional trips to Karafuto and to the great fish warehouses on the Kurile island of Etorofu. His natural bent toward exploration had been stirred by meeting the aged Ino in 1800 and by 1801 Rinsō had made his own map of Yezo. His work was highly considered, and in 1808 the government ordered him to explore Karafuto. In company with Matsuda Denjirō he mapped the east and west coasts of that island and ascertained that it really was an island by charting the Gulf of Tartary still referred to on Japanese maps as the Straits of Rinsō (Rinsō Kaikyō). The winter of 1808, Rinsō spent alone on Karafuto and in the spring of 1809, he crossed to the continent and ascended the Amur as far as the Chinese settlement of Delen. His works, the *Kita Yezo Tsutsetsu* (Discussion of Northern Yezo), the *Tōdatsu Kikō* (Travels in Eastern Manchuria) and some fragments plus his maps are the first really significant contribution to an accurate knowledge of the Hokkaido-Saghalien-Kuriles area with the exception of the maps of Inō. Based on Rinsō's work, the astronomer to the Shogun, Takahashi Sukazaemon, prepared a copper plate map of the Japanese Empire which stood as definitive until a later geographer, Matsuura Takeshirō, resurveyed Yezo and the Kuriles in 1846–48 and published the first modern map of the Kuriles (1850) which he followed later (1859) by publishing a great twenty-nine-section map of Hokkaido and Saghalien.

Unfortunately for the West, the work of the Japanese geographers from Mogami Tokunai to Mamiya Rinsō were not made available outside Japan until 1852. Fr. von Siebold had procured copies of the work of Inō at Nagasaki in 1826. The possession of such documents was a crime and it led directly to his banishment from the Empire. He had also been presented by the aged Mogami Tokunai with the only

[37] Otani, *op. cit.*, 77.

[38] The best work up to this time had been produced by Mogami Tokunai when during 1785–86, he had visited Saghalien and the region around the mouth of the Amur.

[39] Inō's map of Yezo done in 1800 was so good that the government commissioned him to chart the entire coastline of Japan. These surveys were finished in 1821 and under their collective name Dai Nihon Enkai Jissokuroku (A True Survey of the Coasts of Japan) have been the base for all subsequent maps of Japan.

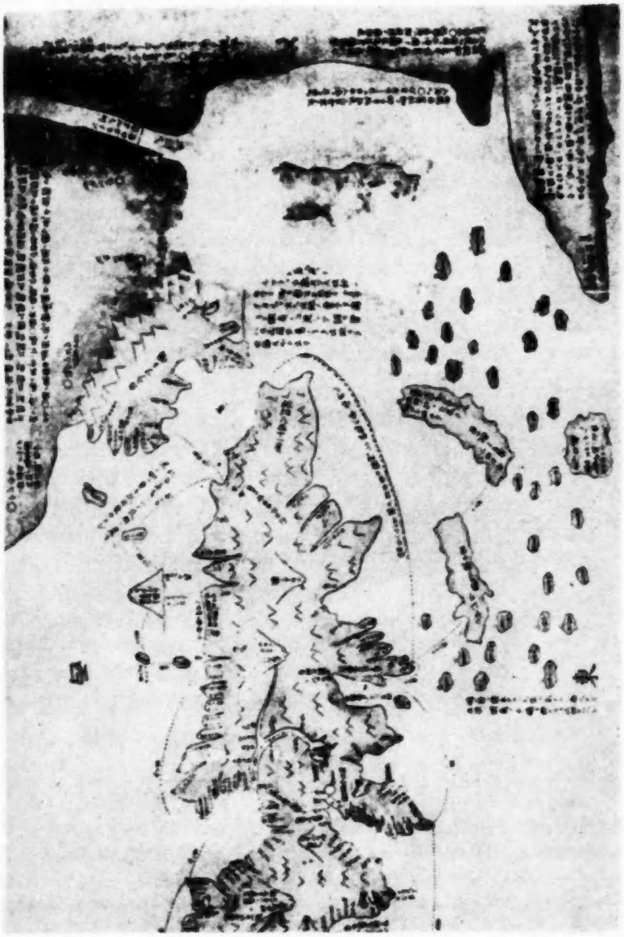

FIG. 3. The northern part of the map of Hyashi Shihei (1785).

existing manuscript copies of Mogami's maps of the Yezo area in 1826 and he had given his word not to reproduce them for twenty-five years. He kept his word. Due to a lack of such information, a series of European voyages were prosecuted late in the eighteenth century that were really voyages of rediscovery. It has already been pointed out that a great deal of time and trouble would have been saved had the log of Vries been available instead of buried in the archives of the DEI. The same fate was suffered by other and later pieces of evidence. The Russian Laxman had made careful charts of the Kuriles and of the coast of Northern Yezo in 1739, but

these charts remained untouched in the archives of Kamchatka until the nineteenth century.[40] When, in 1788, La Perouse discovered that Japan was separated from Yezo by the strait that bears his name, he was merely rediscovering a geographic fact noted not only by Vries and Laxman, but by a Jesuit missionary of the seventeenth century.[41]

In 1721, the Russian envoy to China, Ismailov, had sent to the Tsar Peter I, as a present from the Manchu Emperor K'ang Hsi, a map making clear the fact that Saghalien was an island.[42] About 1755, the Russian explorer and historian Krashnennikov had ascertained the same fact but later voyages made no use of these findings.[43] In 1796–7, James Broughton charted the coasts of Japan, Hokkaido and the Kuriles.[44] Broughton denied the existence of a strait between Saghalien and the mainland as did the later voyage of Krusenstern (1805).[45] The fact of the existence of this strait was to remain with the Russians until the Crimean War. The refusal of the Anglo-French squadron to pass the straits in search of the Russian Pacific squadron may have been due to the reports of 1822 which said that the strait of Tartary had become choked with sand and weeds to such an extent that a small boat would not pass through but the earlier published findings of the Russians should have disproved the fact that such a blockage was possible in a deep and narrow strait.

With the publications of von Siebold in the middle of the nineteenth century Yezo was no longer a mystery. Reliable maps and charts of Hokkaido, Saghalien and the Kuriles could be had. There was ended one of the longest searches in the history of exploration, a search remarkable for the paucity of information produced in relation to the number of explorations made, and for the fact that the reliable foundations layed by the earliest navigators were completely ignored in favor of information that could easily have been proven to have been in error had anyone taken the trouble to check.

[40] Siebold, *op. cit.*, p. 44.

[41] M. Malte-Brun ed., *Universal Geography* (London, 1832), II: 508.

[42] Leon Bagrow, "Die Priorität der Entdeckung des Amur, des Tartarischen Golfs und der Insel Sachalin," *Yamato, Zeitschrift der Deutsch-Japanischen Gesellschaft,* III (1931), 84.

[43] S. Krashnennikov, *Histoire du Kamtschatka, des Isles Kuriles, Et Des Contries Voisines,* trans. M. E. (Lyon, 1767). 2 vol. *et passim,* particularly II: 33–37.

[44] Broughton stated that he had no chart to aid him but that of Cook's third voyage! William Robert Broughton, *A Voyage of Discovery to the North Pacific Ocean . . . in the Years 1795, 1796, 1797, 1798* (London, 1804), 302.

[45] The value of any of these later voyages is doubtful. Isaac Titsingh, the learned Orientalist, in his preface to the translation of Ieso-ki (Description of Yezo) by Kannemon states that in 1814 the coast of Hokkaido was not known despite the voyages of Krusenstern, La Perouse and Broughton. *Annales des Voyages de la Geographie et de l'Histoire,* XXIV (1814), 147–148.